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THE DECEMBER SCIENTIFIC MONTHLY

EDITED BY J. MCKEEN CATTELL

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THE SCIENCE PRESS

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Mechanics for Students of Engineering. HENRY CREW and KEITH K. SMITH. xvi + 371 pp. Illustrated. $4.00. Macmillan Company.

The aim of the authors has been to treat mechanics in a manner which is interesting, concrete and historical. Accordingly the treatment begins with the earliest-discovered principles of statics and continues to the more modern developments of the science.

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The author here seeks to gather all the evidence concerning this small cat-fish which lives in the Amazon River and its tributaries, and which is the only known vertebrate parasite of man. Dr. Aldred S. Warthin, who is professor of pathology at Michigan, contributes the foreword.

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This is a volume in the series "Economic and Social History of the World War" published for the Carnegie Endowment for International Peace. It deals, in particular, with the agriculture and land settlement in Russia.

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In this volume four scientists, members of the faculty of the University of Minnesota, report the application of quantitative methods to the study of mankind. These essays were presented first as lectures under the auspices of Sigma Xi.

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The author first tests the Darwinian theory of natural selection by mathematical and statistical methods. He then goes on to deal with biological problems as they affect the human race, and includes an explanation of the rise and fall of civilizations.

The Wild Grizzlies of Alaska. JOHN M. HOLZWORTH. xxi + 417 pp. Illustrated. $5.00. G. P. Putnam's Sons.

A story of the grizzly and big brown bears of Alaska, their habits, manners and characteristics, together with notes on mountain sheep and caribou collected by the author for the U. S. Biological Survey.

Comets. CHARLES P. OLIVIER. x + 245 pp. Illustrated. $3.50. Williams and Wilkins Company.

This book has been written for readers of general intelligence. It covers briefly the history of comets and the present theories of origin, constitution, changes and dissolution of comets. Mathematical discussions are omitted.

THE SCIENTIFIC MONTHLY

DECEMBER, 1930

A BOTANICAL TRIP TO SOUTH AND EAST AFRICA

By Dr. A. S. HITCHCOCK

BUREAU OF PLANT INDUSTRY, UNITED STATES DEPARTMENT OF AGRICULTURE

THE British Association for the Advancement of Science met in South Africa last summer (1929) in cooperation with the South African Association for the Advancement of Science. The writer was invited to attend the meeting

THE HERBARIUM OF THE ROYAL BOTANIC GARDEN AT KEW

which was held at Capetown, from July 22 to 27, and at Johannesburg, from July 30 to August 5.

Leaving America early in June, I went to London, taking from there the *Llandovery Castle* of the Union Castle line. About ten days were spent in London studying types of grasses at Kew. On board the *Llandovery Castle* were about 160 scientists bound for the scientific meetings. Other boats brought their quotas. There were in attendance at the meetings only about a dozen from the United States and Canada and a sprinkling from the Continent of Europe.

The *Llandovery Castle* called at the island of Teneriffe where passengers landed for a few hours, taking drives over the hills and going through some of the parks of Santa Cruz. The famous peak of Teneriffe (12,152 feet) was hidden from view by a covering of clouds. Botanists were interested in a large specimen of the dragon tree (*Dracaena draco*) said to be very old. Another prominent feature of the landscape was a succulent spiny species of Euphorbia (*E. canariensis*) which resembled a cactus. The plants are tree-like with a single trunk, but the branching resembles that of a candelabra as in many of our southwestern and Mexican species of cactus.

THE GREAT DRAGON TREE

(*Dracaena draco*) ON TENERIFFE. DR. RENDLE, OF THE MUSEUM OF NATURAL HISTORY AT SOUTH KENSINGTON, HAS JUST TAKEN A PICTURE OF THE SAME TREE.

THE ISLAND OF ASCENSION

WITH THE CABLE STATION, TAKEN FROM THE STEAMER AT ANCHOR.

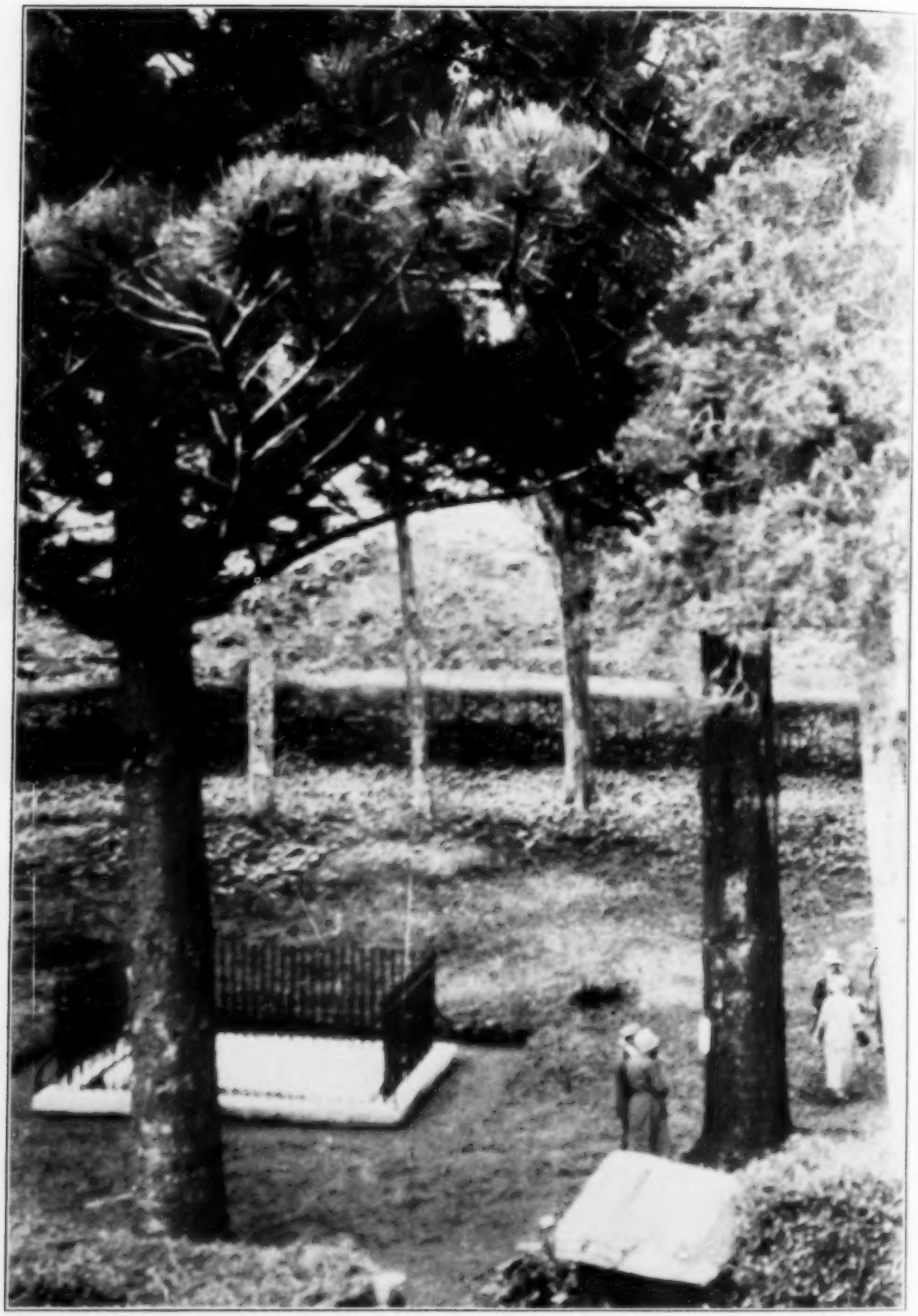
THE LANDING PLACE ON THE ISLAND OF ST. HELENA

THE COAST IS PRECIPITOUS AND THE WHOLE ISLAND IS MOUNTAINOUS.

The weather now began to be warm but was uncomfortable for only two or three days. We were in sight of land as we rounded the westernmost coast of Africa. The next land sighted was the small island of Ascension, which lies about eight degrees south of the equator. The island has an area of only 38 square miles, but the central part rises to a height of 2,820 feet. We were unable to land as the steamer was anchored a considerable distance out and the facilities for landing were too meager to accommodate so many passengers. The island is quite barren although it is said that there is vegetation near the summit

THE INTERIOR OF ST. HELENA

TERRACED FIELDS OF NEW ZEALAND FLAX (*Phormium tenax*) IN THE MIDDLE DISTANCE. A COARSE FIBER IS OBTAINED FROM THE LEAVES. NEW ZEALAND FLAX IS THE CHIEF EXPORT OF THE ISLAND.



THE GRAVE OF NAPOLEON

ON THE ISLAND OF ST. HELENA. THIS PLOT OF GROUND IS FRENCH TERRITORY THOUGH THE ISLAND BELONGS TO GREAT BRITAIN. THE BONES OF NAPOLEON HAVE BEEN REMOVED TO PARIS.

BOTANISTS ON AN EXCURSION

THROUGH THE CAPE PENINSULA SOUTH OF CAPETOWN.

because of the clouds and mists there. Great Britain maintains a cable station on the island.

Seven hundred miles further to the southeast, and still within the tropics, lies the beautiful island of St. Helena, known to Americans chiefly as the home of Napoleon from 1816 till his death in 1821. The rugged mountainous island has an area of 47 square miles, the highest point being 2,700 feet above the sea. Several hours were spent upon the island by the passengers, many of whom visited Longwood, the residence of Napoleon. Nearby is the grave of Napoleon, whose body, however, has been removed to Paris. The rainfall is sufficient to give a fairly ample flora and the botanists were very busy. The chief export crop of St. Helena is New Zealand flax (*Phormium tenax*), the leaves of which produce a coarse fiber useful for cordage and bagging.

The weather had been cool since leaving the equator and became much cooler as we approached Capetown.

TUSSOCKS OF A SPECIES OF THE FAMILY RESTIONACEAE

ALLIED TO THE SEDGES AND GRASSES. THIS FAMILY IS WELL REPRESENTED IN SOUTH AFRICA.

A NEW BUILDING BEING THATCHED WITH THE STEMS OF RESTIONACEAE. THE LOWER ROOFS SHOW THE THATCHING COMPLETED. THESE PLANTS ARE COMMONLY USED FOR THATCHING IN THE CAPE REGION.

The arrangements for the reception of visiting scientists were unusually good. According to a plan worked out in advance there was a host for each guest. My host, an American in residence, looked after my welfare from the time the steamer arrived until I left for Johannesburg. The host system was used at the latter city also. I have never attended a scientific meeting anywhere in the world where guests were so delightfully entertained and where the plans for entertainment were carried through with such perfection and punctuality.

The sessions of the Botanical Section of the association were well attended, and there was opportunity to meet nearly all the South African botanists. The sessions of the association were held in the fine large buildings of the University of Capetown. The writer read by invitation a paper on "The Relation of Grasses to Man."

The National Botanic Gardens at Kirstenbosch were of great interest to visiting botanists. The garden is near Capetown, the upper part running up on the slopes of Table Mountain to the top (3,500 feet). A large part of the garden is given over to the preservation of the indigenous vegetation, which has great botanical interest because the flora of the Cape region is peculiar and striking, and for the most part different from that of any other part of the world. Within the garden is the Bolus Herbarium, containing a large collection of South African plants.

A SHRUB OF THE FAMILY PROTEACEAE (*Protea grandiflora*), ONE OF THE PECULIAR FAMILIES CHARACTERISTIC OF THE CAPE REGION.

THE BOLUS HERBARIUM IN THE KIRSTENBOSCH BOTANIC GARDEN, CAPETOWN. THE BOLUS HERBARIUM IS ONE OF THE IMPORTANT COLLECTIONS OF PLANTS IN SOUTH AFRICA.

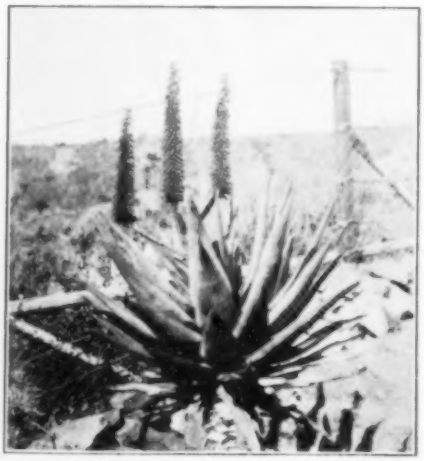
There is an important herbarium also at the South African Museum, adjoining the Municipal Botanic Gardens of Capetown.

PROTEACEAE (*LEUCOSPERMUM LEPTOCARPUM*)
ON THE SLOPES OF TABLE MOUNTAIN NEAR CAPETOWN.

Excursions for botanists to the surrounding regions furnished an opportunity for examining the wonderfully rich flora. It is said that the Cape Peninsula supports a flora of about 2,500 species of flowering plants. The Cape Peninsula is a narrow tongue of land projecting southward from Capetown for about 30 miles, the central core being Table Mountain and its extension to the Cape of Good Hope at the southern extremity. The family Proteaceae forms a conspicuous element of the flora. There are large numbers of monocotyledonous plants with bulbs and corms. Succulents abound, the genus Mesembryanthemum and its allies being particularly noticeable.

Although the time of our visit (August) was the winter season a large number of plants were found in bloom. The annual rainfall at Capetown is about 25 inches. Frost is unknown in the city but occurs at higher altitudes.

The sessions of the second week of the



A SUCCULENT (*ALOË FEROX*)
IN THE KAROO GARDEN AT WHITE HILLS. THIS IS A BRANCH OF THE MAIN GARDEN AT CAPETOWN.

NATIVE CHILDREN AT A RAILWAY STATION

A FEW OF THE FINE NEW BUILDINGS

OF THE WITWATERSRAND UNIVERSITY AT JOHANNESBURG. THE BOTANICAL DEPARTMENT IS IN THE BUILDING TO THE LEFT.

association were held at Johannesburg. This is the largest city in South Africa, the population being about 175,000 white and 150,000 colored. It lies on a plateau at an elevation of 6,000 feet about 1,000 miles north and east of Capetown. Johannesburg is a modern metropolis reminding an American of his middle western cities. The Witwatersrand University has several large and imposing buildings. The botanical department is well equipped and has a good herbarium. In this region I observed for the first time a beautiful lawn grass (*Cynodon transvaalensis*) allied to our Bermuda grass but with a much finer texture and a darker green color. My host and hostess at Johannesburg, both Americans, looked after my welfare in that city in the same efficient and sympathetic manner that characterized the oversight of guests during their entire stay in South Africa.

About 40 miles north of Johannesburg lies Pretoria, a city of about 100,000 inhabitants including the colored

A FINE LAWN OF TRANSVAAL GRASS

(*Cynodon transvaalensis*) IN FRONT OF THE HOUSE OF MY HOST, MR. WARTENWEILER, AT JOHANNESBURG. THE GRASS IS ALLIED TO OUR BERMUDA GRASS BUT IS MUCH FINER IN TEXTURE.

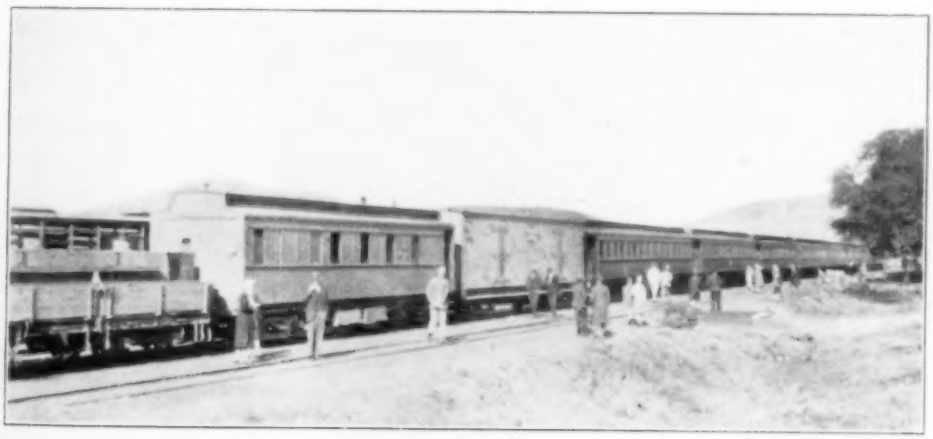
THE HERBARIUM

OF THE BUREAU OF PLANT INDUSTRY AT PRETORIA (THE NATIONAL HERBARIUM).

population. This is the administrative capital of the Union of South Africa. Here is the National Department of Agriculture, one division of which, the Division of Plant Industry, includes the botanical investigations of the government. The National Herbarium contains a large collection of African plants. At the Experiment Station there is a good collection of native forage grasses grown in plots.

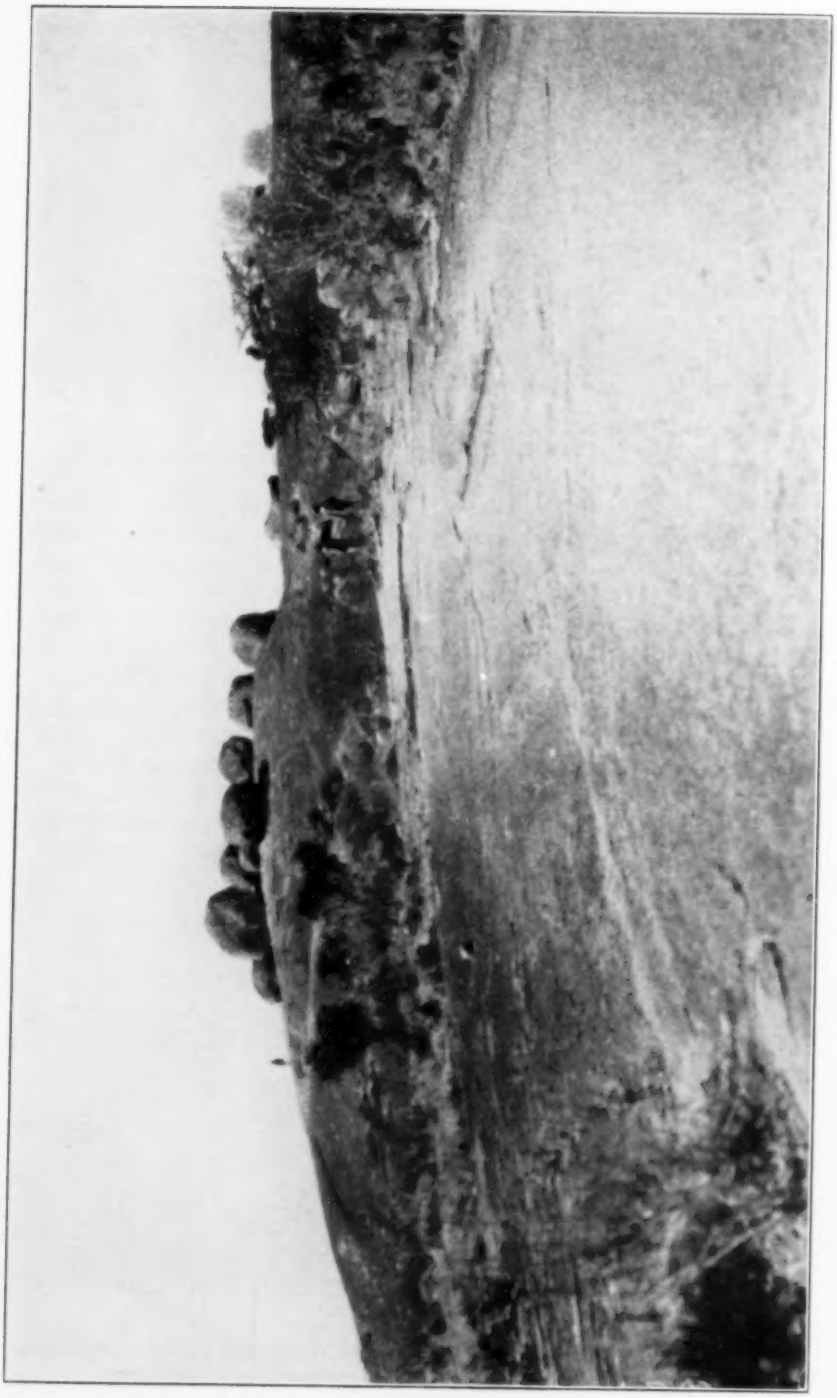
Considering the age and development of the Union of South Africa, the progress made in science and education is striking. The buildings of the universities and museums are large, modern and of fine appearance. The botanical departments are well equipped and well staffed. There are several herbaria, the largest of which are the National Herbarium, the Bolus Herbarium and the Herbarium of the South African Museum at Capetown.

The visiting scientists were offered the



THE TRAIN

ON WHICH ONE OF THE EXCURSION PARTIES SPENT 12 DAYS FROM JOHANNESBURG TO VICTORIA FALLS AND BEIRA.



MOTOPO HILLS NEAR BULOWAYO

THE BOULDERS ARE ON THE HIGHEST POINT, "WORLD'S VIEW," OVERLOOKING THE COUNTRY FOR MILES AROUND. RHODES'S GRAVE LIES AMONG THE BOULDERS.

CHAPARRAL IN SOUTHERN RHODESIA

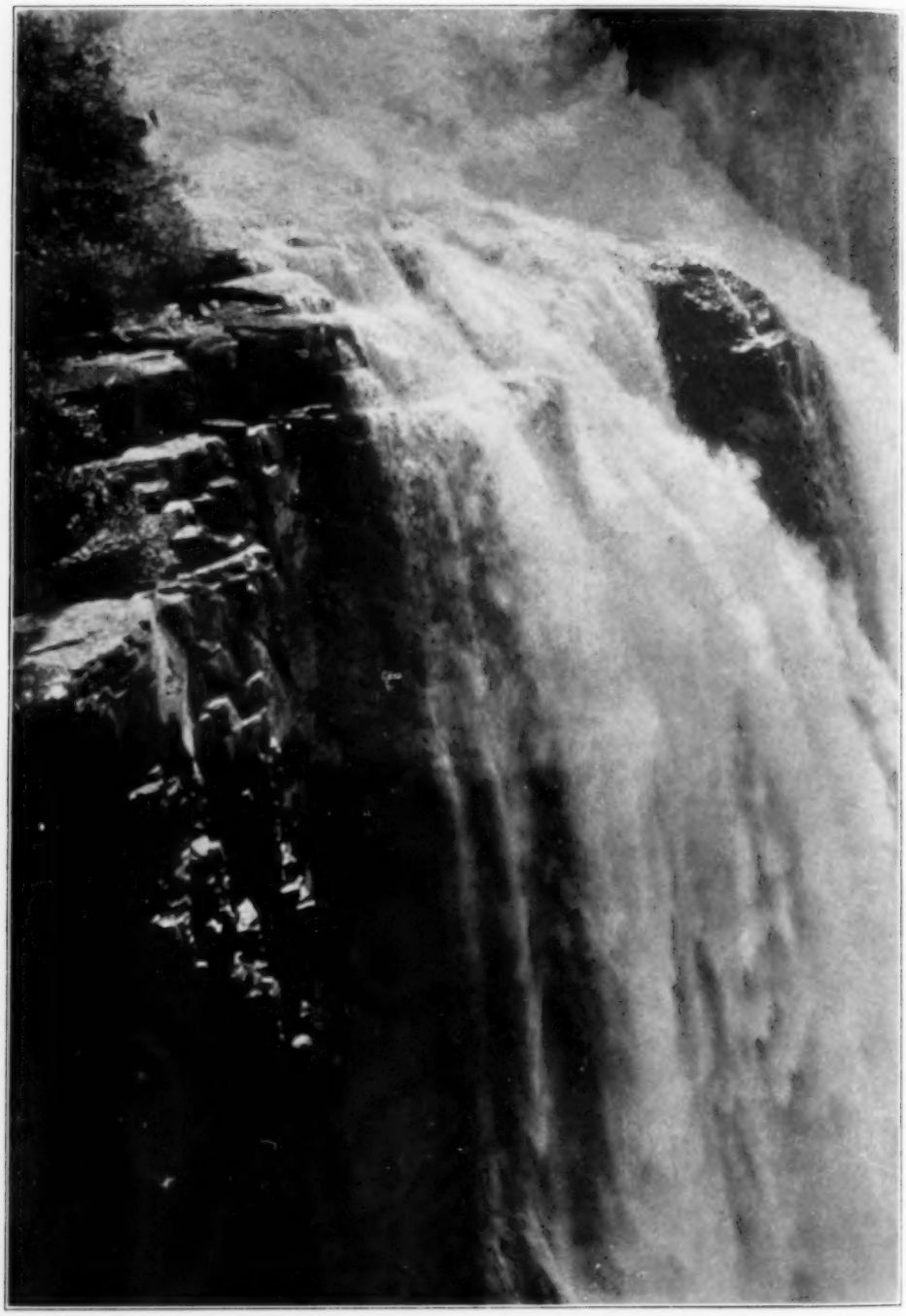
THIS ARID REGION REMINDS ONE OF PARTS OF OUR SOUTHWEST.

privilege of participating in several excursions. Many botanists, including myself, left Capetown Saturday night by special train to examine the flora of the Karoo, an arid region between Capetown and Johannesburg. Two days were spent in the Karoo under the leadership of Professor Compton, director of the National Botanic Gardens. The Karoo formation includes a large part of the interior of the Cape Province. It is an arid plateau, mostly 1,500 to 3,000 feet in altitude with a rainfall usually under 12 inches. The vegetation is rich in succulents.

At Whitehill, a station on the railroad through the Karoo, is the Karoo Garden, a branch of the National Botanic Gardens at Kirstenbosch (Capetown). This garden is a natural preserve of 40 acres at an altitude of 3,000 feet, with an annual rainfall of about six inches. The garden is rich in native succulents. The visiting botanists spent several

THE GRAVE OF CECIL RHODES

A PROMINENT FIGURE IN SOUTH AFRICA A FEW YEARS AGO. THE TOP OF THE GRAVE IS SEEN AT THE BASE AMONG THE BOULDERS.



A SMALL PART OF THE VICTORIA FALLS

THE FALLS ARE 420 FEET HIGH AND 6,000 FEET WIDE.

NATIVE HUTS NEAR VICTORIA FALLS

hours studying the strange and interesting flora. Here were plants related to Mesembryanthemum consisting of two hard succulent leaves, the whole resembling in form and color the small globular stones among which they grew.

On the way to Johannesburg the party stopped at Kimberley, where the diamond mines were visited. We had the unusual opportunity of viewing at the offices great piles of diamonds worth hundreds of thousands of dollars.

After the close of the scientific meetings I joined an excursion by special train to Victoria Falls. For two weeks we lived on the train, which was provided with sleeping accommodations, dining car and baggage cars where the heavy baggage was always accessible. The management of this excursion was all that could be desired.

At Bulowayo (Southern Rhodesia) we went by motor to the Motopo Hills, a park with much beautiful natural scenery. The highest point, World's View, is a bare rounded hill, from which one obtains a view over the surrounding country for many miles in all directions.

TRANSPORTATION IN SOUTHERN RHODESIA

TEAMS OF 8 AND 9 YOKE OF OXEN WERE COMMONLY USED BY THE BOERS IN THE EARLY DAYS BEFORE THE ADVENT OF THE MODERN MOTOR TRUCK AND ARE STILL TO BE SEEN IN REMOTE REGIONS. THIS PICTURE WAS TAKEN AT FORT VICTORIA.

THE HOTEL AT ZIMBABWE

12 MILES SOUTH OF FORT VICTORIA. THE BUILDINGS ARE IN IMITATION OF THE NATIVE HUTS. THE ZIMBABWE RUINS ARE NEARBY.

The hill is topped by several large boulders among which is set the grave of Cecil Rhodes and his comrade Jameson. The archeologists were interested in a cave containing Bushman paintings.

Two days were spent at Victoria Falls in the Zambesi River. These remarkable falls are one of the wonders of the world. They are 420 feet high and about 6,000 feet wide. The peculiar contour is such that it is impossible, except from the air, to get the falls in a single view. The vast amount of spray also interferes. For these reasons the Niagara Falls, with which the Victoria Falls are often compared, are more impressive because of the good view and the regularly large volume of water passing over. The seasonal variation of the amount of water passing over the Victoria Falls is great. During high

EUPHORBIAS NEAR ZIMBABWE RUINS

IN HABIT THESE PLANTS RESEMBLE CACTUSES, THOUGH BELONGING TO AN ENTIRELY DIFFERENT FAMILY.

THE ACROPOLIS

A PREHISTORIC FORTIFICATION THAT IS ON A HILL NEAR THE OTHER RUINS AT ZIMBABWE. THE GREAT BOULDERS HAVE BEEN REINFORCED BY ARTIFICIAL WALLS.

water the spray fills the canyon and little can be seen. The peculiarity of the Victoria Falls is the position and contour of the canyon. The river flows through a great lava bed and the general level of the country is the same above and below the falls. The chasm into which pours the water from the falls is at right angles to the flow of the river, hence the falls appear to plunge toward a solid wall in front. Through this wall, at a narrow deep gorge, flows the entire river. It seems almost unbelievable that so much water can pass through such a narrow gorge. The railroad bridge that spans the canyon just below this gorge gives the observer a good view in both directions. The canyon below the bridge makes two hairpin turns and then extends down the river

A MAIZE FIELD NEAR FORT VICTORIA

MAIZE OR CORN, KNOWN IN SOUTH AFRICA AS MEALIES, IS EXTENSIVELY CULTIVATED, ESPECIALLY IN TRANSVAAL AND RHODESIA. THE FIELD SHOWS THE WINTER CONDITION AFTER THE EARS HAVE BEEN GATHERED.

SACKS OF "MEALIES" (MAIZE) READY FOR SHIPMENT

NEAR FORT VICTORIA (FARM OF MR. HALLIDAY). THE EARS ARE HUSKED BY MACHINERY; THE SHREDDED HUSKS ARE IN THE BACKGROUND.

for 40 miles. Victoria Falls were discovered and made known to the world by David Livingstone in 1855.

In the vicinity of Victoria Falls we saw our first baobab tree (*Adansonia digitata*). These curious and somewhat ungainly trees were bare of leaves at the time of our visit. They have a trunk entirely out of proportion to the branches. The girth may be as much as 75 or even 100 feet while the height may be 50 or 60 feet. These trees were seen again in East Africa, and the species extends across Africa to Senegal.

Our next stop of importance was at the Zimbabwe ruins 12 miles south of Fort Victoria, the nearest railroad station.

The buildings were evidently erected for the protection of those engaged in the gold industry. From the discovery of crucibles and other implements, it is obvious that ore was smelted within the walls. It seems probable that Great Zimbabwe was the chief inland stronghold of

"MEALIES" (MAIZE) IN SACKS

AT A RAILWAY STATION READY FOR SHIPMENT.

THE HOME OF THE DIRECTOR

OF THE AMANI AGRICULTURAL INSTITUTE, THE CENTER OF SCIENTIFIC RESEARCH IN TANGANYIKA (FORMERLY GERMAN EAST AFRICA).

those who came in search of the precious metal.[1]

The archeologists were greatly interested in these ruins, some of which were in a good state of preservation. The age of the ruins is not known but they are thought to date within the Christian era.

PETER'S HUT

(ALT. 11,500 FT.) THE SECOND RESTHOUSE ON THE TRAIL UP MT. KILIMANJARO. THIS IS IN THE ALPINE REGION.

[1] "The South and East African Year Book and Guide for 1929," page 458.

A large farm near Fort Victoria was visited by many of our party. This farm gives an example of the modern methods of agriculture practiced in Southern Rhodesia. Agricultural machinery is freely used, and it was noted that this came mostly from America. We were told that 95 per cent. of the automobiles in South Africa were of American make. The conditions in the rural districts of South Africa are similar to those found in rural America. Heavy powerful machines are needed; those coming from England are too light.

BAOBAB TREE (*ADANSONIA DIGITATA*)

NEAR MOSHI AT THE BASE OF MT. KILIMANJARO. THE NATIVES HAVE PLACED IN THE BRANCHES SMALL HOLLOW LOGS FOR BEEHIVES.

It was interesting to note that four of the most important food plants cultivated in Rhodesia and Transvaal are of American origin: maize (called mealies in South Africa), sweet potato, white potato and casava. The last is a staple food plant of Brazil and other parts of tropical America. The part used is the fleshy roots rich in starch. It comes into the American market as tapioca.

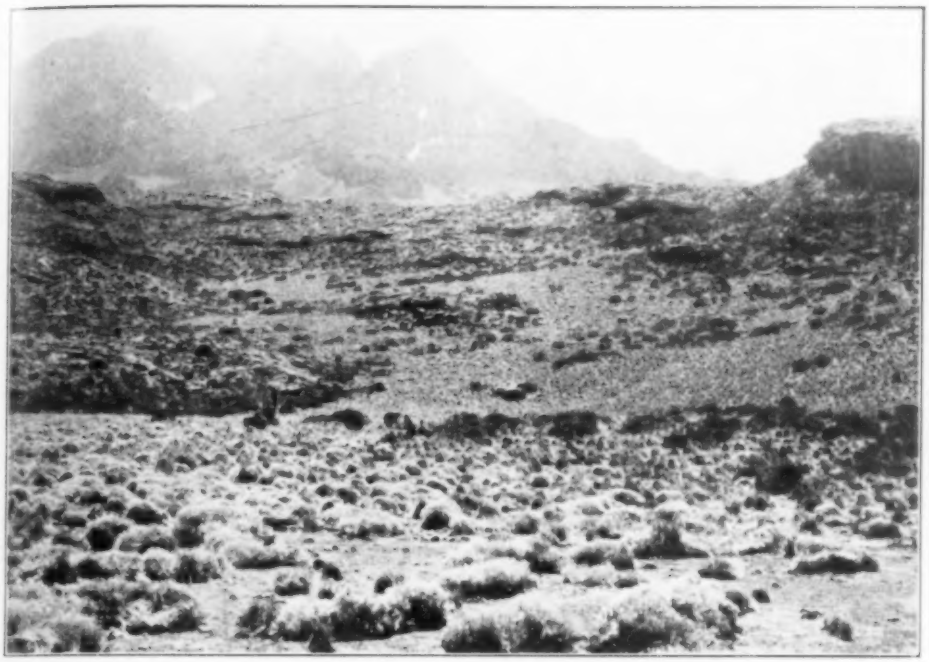
The excursion ended at Beira in Portuguese East Africa where we embarked on a steamer for the north. Some of the party remained on the steamer till it reached London. Others

KIBO PEAK OF MT. KILIMANJARO

FROM ABOUT 14,000 FT., THE LIMIT OF VEGETATION AT THIS POINT.

TRAIL UP MT. KILIMANJARO

PASSING THROUGH THE SCRUB ZONE ABOVE THE FOREST. THE PORTERS ARE RESTING.



MAWENZI, THE EASTERN PEAK OF MT. KILIMANJARO
(ALT. ABOUT 17,000 FT.), TAKEN FROM 14,000 FT. ELEVATION.

ALPINE REGION ON MT. KILIMANJARO
ABOVE PETER'S HUT, ALTITUDE ABOUT 12,000 FEET. A BOG WITH SCATTERING PLANTS OF *Senecio Johnstonii*.

BISMARK HUT

(ALT. 8,500 FT.) THE FIRST RESTHOUSE ON THE TRAIL UP MT. KILIMANJARO NEAR THE UPPER LIMIT OF THE RAIN FOREST.

disembarked at Mombasa and went up to Nairobi. A few of us, including Mr. Cotton, keeper of the Kew Herbarium, landed at Tanga. On the way we stopped a short time at Dar-es-Salaam and Zanzibar. Dar-es-Salaam is the capital of Tanganyika, formerly known as German East Africa, now a mandated territory of Great Britain. At Zanzibar the visitors were by previous arrangements assigned to hosts. We arrived about 4.00 P. M. and left at midnight. My host took me in his automobile at once into the country where I could collect grasses. He returned about dark to carry me back to the city. Without this fortunate cooperation I should not have obtained any grasses here.

The island of Zanzibar, with the neighboring island Pemba, is a British protectorate. The chief crop is cloves. At Zanzibar the visitors had their first view of Arabic architecture and culture.

Tanga is the northernmost port of Tanganyika. About 40 miles from Tanga lies the Amani Agricultural In-

NATIVE HUT NEAR MARANGU

THE VILLAGE FROM WHICH ONE COMMENCES THE ASCENT OF MT. KILIMANJARO.

stitute, in the mountains at an elevation of about 3,000 feet. The institution is well equipped for doing scientific work. There are among other facilities a good botanical laboratory and a herbarium. This station was formerly the center of scientific work in German East Africa. After making collections at Amani Mr. Cotton and I went to Moshi by rail on our way to Kilimanjaro. We found that the ascent of the mountain should be made from the village of Marangu about

SENECIO COTTONI

A GIANT SENECIO ABOUT 14 FEET HIGH (THE BASE NOT VISIBLE) AMONG ROCKS ON MT. KILIMANJARO AT ABOUT 13,000 FEET ALTITUDE.

A NATIVE WOMAN

CARRYING A BUNDLE OF GRASS FOR FORAGE. ROAD NEAR MT. KENYA. FIELDS OF MAIZE IN BACKGROUND.

10 miles northeast of Moshi. The ascent of Kilimanjaro is easy up to the limit of vegetation. There is a good trail with a gradual grade. We started from the pleasant little hotel in the morning on mules accompanied by a cook and four porters on foot. The first night was spent at Bismark Hut at about 8,000 feet. The trail led through cultivated land for an hour or so and then through forest. Bismark Hut is a stone rest house with three rooms. The second day we went to Peter's Hut at about 12,000 feet. We soon emerged from the forest after leaving Bismark Hut and passed over grassland with some scrub or bush. Peter's Hut, on an open rocky slope, is a galvanized iron resthouse with one fairly large room. From this point the snow-capped top of Kibo peak is visible over a near-by ridge. The third day was spent in collecting in the vicinity of Peter's Hut, though I went up to about 14,000 feet, which at that point was the limit of vegetation. Here was a beautiful view

GOVERNMENT HOUSE, NAIROBI

THE RESIDENCE OF THE GOVERNOR. A FINE LAWN OF KIKUYU GRASS (*Pennisetum clandestinum*). THE GRASS IN THE FOREGROUND HAS BEEN RECENTLY SET OUT. TO THE LEFT THE GRASS HAS FILLED IN TO A FIRM SOD.

of Kibo, the western peak of Kilimanjaro, with its rounded snow-covered top, bright and distinct in the morning, and said to be the highest peak in Africa (19,710 feet). To the right lay the eastern peak, Mawenzi (about 17,000 feet), the jagged summit without a snow cap. On the fourth day we descended to Marangu.

The grasses of the region above timber line on Kilimanjaro are temperate or alpine species belonging to such familiar genera as *Poa*, *Festuca*, *Bromus*, *Koeleria* and *Agrostis*.

The journey was continued by rail to Nairobi *via* Voi. On the plains approaching Nairobi vast quantities of game were to be seen from the car win-

THE WIVES OF A NATIVE CHIEF NEAR MT. KILIMANJARO

NOTE THE GREAT QUANTITY OF BRASS WIRE AROUND THE ARMS, THE BRASS SPIRALS AROUND THE NECKS AND THE HEAVY ORNAMENTS IN THE DISTENDED EAR LOBES SUPPORTED BY BAND OVER THE TOP OF THE HEAD.

ANTHILLS ALONG THE SHORE OF LAKE VICTORIA
AT ENTEBBE. SUCH HILLS ARE COMMON IN TROPICAL AFRICA.

dows, zebras, ostriches, giraffes and various kinds of antelopes and gazelles. Nairobi is a city of about 15,000 on a great grassy plateau at an altitude of about 5,500 feet, distant 330 miles from Mombasa on the coast. It is from this city that most of the hunting trips (safaris) for big game are made.

A two-day automobile trip was made to Nyeri and Nanyuki lying north of Nairobi near Mt. Kenya (17,050 feet, snow-capped).

Through the courtesy of government officials a trip into Uganda was arranged for me. I first went by rail to Kisumu on Lake Victoria. Leaving Kisumu at 10:00 A. M. by a comfortable steamer I arrived at Entebbe the next morning. The altitude of the beautiful Lake Victoria is about 3,700 feet. The following morning I went by automobile to Kampala, the center of agricultural research in Uganda. There is here an agricultural department with a botanical laboratory. Again by automobile I was taken to Jinja, the terminus of the rail-

THE RESIDENCE OF THE AMERICAN CONSUL AT NAIROBI

RIPON FALLS OF THE NILE RIVER

JUST BELOW LAKE VICTORIA. THIS IS CALLED THE SOURCE OF THE NILE.

road on Lake Victoria. At this point is the outlet of the lake to the Nile system. This outlet is called the source of the Nile. The return to Nairobi was made by rail, stops of one day each being made at Eldoret, Nakuru and Naivasha. The collecting at Eldoret and Nakuru was unusually good. The grassy plains and rocky hills were ungrazed and the grass flora was very rich. The distribution of the rainfall was such that all grasses appeared to be brought to flowering at just the time of my visit.

In Uganda the most conspicuous grass was elephant grass (*Pennisetum purpureum*), a robust gregarious plant growing to the height of 8 to 10 feet or even as much as 20 feet.

Nakuru and Naivasha lie in the Great Rift Valley, a fault or rift that has been traced more or less distinctly from Lake Nyasa to Abyssinia and even further, according to some. The valley is distinct in Kenya, 40 to 60 miles wide, with a flat floor and escarpments on each side of about 1,500 feet.

Kenya has been known as a great game country. The amazing numbers of herbivorous animals mean on the one hand a large number of predatory carnivores and on the other hand a region rich in grasses to furnish the necessary grazing. Thus the great grassy plateau is a paradise for an agrostologist.

ELEPHANT GRASS
(*PENNISETUM PURPUREUM*)

IN UGANDA ON THE ROAD BETWEEN KAMPALA AND JINJA. THE GRASS IS ABOUT 12 FEET HIGH. ELEPHANT GRASS IS A COMMON AND CONSPICUOUS GRASS IN TROPICAL AFRICA. IT IS USED FOR FORAGE AND HAS BEEN INTRODUCED INTO TROPICAL AMERICA WHERE IT GIVES PROMISE OF SUCCESS.

A THORNY ACACIA TREE

COMMON ON THE PLAINS OF KENYA. THE PICTURE WAS TAKEN NEAR NAIVASHA IN THE RIFT VALLEY.

The taller grasses of the dry plains belong mostly to the tribe Andropogoneae and especially to the genera *Hyparrhenia* and *Cymbopogon*. The genus *Eragrostis* belonging to another tribe (Festuceae) is the richest in species of any of the genera observed.

In East Africa one of the native species has been utilized as a lawn grass. This is commonly known as Kikuyu grass (*Pennisetum clandestinum*). It produces vigorous stolons and spreads

GALVANIZED HUTS OF WORKMEN NEAR NAIVASHA

THE RIFT VALLEY NEAR NAIVASHA

THE VALLEY IS FLAT, 40 TO 60 MILES WIDE, WITH A DISTINCT ESCARPMENT ON EACH SIDE.

ADEN, SOUTHWEST ARABIA

IN A VERY ARID REGION. THE PICTURE WAS TAKEN A SHORT TIME BEFORE SUNRISE.

SUEZ CANAL

A STATION ON THE EAST BANK. A LARGE HERD OF CAMELS MAY BE SEEN.

readily when pieces of these stolons are set out. The lawns of this grass are beautiful in color, evenness and density. The texture is somewhat coarser than Bermuda grass (*Cynodon Dactylon*) but otherwise the Kikuyu grass is ideal for tropical regions. It appears to withstand a moderate amount of frost. The limits for availability of this grass as to latitude and altitude have not yet been determined.

On September 28 I sailed for London from Mombasa. Having about two hours to wait before the departure of the steamer, I made a good collection of grasses from a hill near the dock. The return journey was through the Red Sea to Marseilles and across France and the channel to London. A quick change to a transatlantic boat brought me to New York about one month from the time I left Mombasa.

AN ENTOMOLOGICAL SHEEP IN WOLF'S CLOTHING

By Professor WILLIAM L. DOLLEY, Jr.,

PROFESSOR OF BIOLOGY, UNIVERSITY OF BUFFALO

and HENRY G. HAINES[1]

ONE of our commonest insects, the drone fly, *Eristalis tenax*, passes its adult existence unrecognized and avoided by most people because of its extraordinary resemblance to the honey bee. There are few insects except the silkworm and the honey bee that have a historical record equal to that of this interesting creature. Its story begins in the dusk of prehistoric times.

Because it is one of the organisms which have acquired the habit of living in man's vicinity and of following him over the face of the earth, and because recent work has shown it to be splendidly adapted for use in certain types of physiological and genetic investigations, it is important that the known facts about the drone fly be summarized. In the following pages is given the historical record of this remarkable insect and what is known about its life history and distribution.

HISTORICAL RECORD

So close is the resemblance between the drone fly and the honey bee that one is not surprised to read the following words of the great French naturalist, Réaumur: "I scarcely ever dare to take in my hand without hesitating one of these two-winged flies (*Eristalis tenax*)." Nor is it surprising to know that Heliogabalus, a Roman emperor of the third century A. D., was accustomed to play practical jokes on his friends by frightening them in sending them vessels filled with living drone flies. One can imagine the consternation and the reflexes produced by the perfectly harmless flies whose resemblance to angry bees is so marked.

This similarity between drone flies and honey bees is in part responsible for a belief, the Bugonia myth, which lasted for thousands of years, namely, that the bodies of dead animals give rise to honey bees. The earliest record we have of this myth is found in the Bible (Judges xiv). Samson on coming upon the carcass of a lion which he had killed previously is said to have found in the carcass a swarm of bees and honey, to have eaten some of the honey and to have later proposed a riddle, "Out of the eater came forth meat, and out of the strong came forth sweetness." The very fact that he proposed this riddle shows that this belief was wide-spread at this time. It prevailed in North Africa and in some parts of Asia, continued through the middle ages and found expression in the sixteenth and seventeenth centuries.

According to Ovid, Aristaeus, who first taught men to plow and to keep bees, was appealed to for help by certain shepherds who had lost all their swarms of bees. He told them that they might regain their riches from a slaughtered bullock, if they raised suitable altars to the gods, observed certain ceremonies and treated the carcass in a particularly described manner, when swarms of oxen-born bees would enable them to replenish their stores.

[1] The junior author collaborated in this work while an honors student in the University of Buffalo.

Many recipes for making bees are found in the works of ancient authors. Perhaps the most explicit is that given by Florentinus in describing the process used by Juba, King of Northwestern Africa, who died in A. D. 23:

> Build a house, ten cubits high, with all the sides of equal dimensions, with one door and four windows, one on each side: put an ox into it, thirty months old, very fat and fleshy: let a number of young men kill him by beating him violently with clubs, so as to mangle both flesh and bones, but taking care not to shed any blood: let all the orifices, mouth, eyes, nose, etc., be stopped up with clean and fine linen impregnated with pitch: let a quantity of thyme be strewed under the reclining animal, and then let windows and doors be closed and covered with a thick coating of clay, to prevent the access of air or wind. Three weeks later let the house be opened, and let light and fresh air get access to it, except from the side from which the wind blows strongest. After eleven days you will find the house full of bees, hanging together in clusters, and nothing left of the ox but horns, bones, and hair.

Vergil's description of the process is similar. In his "Georgics" he also says:

> De putri viscere passim
> Florilegae nascuntur apes.

One writer specified that the ox should be buried with projecting horns, so that when the horns were cut off the bees could emerge from the cut ends.

Pliny (b. A. D. 23) thought it was only necessary to use the entrails of an ox covered with dung.

Aelian of Praeneste, a Greek writer of about A. D. 200, claims as one of the useful qualities of oxen "that their remains are capable of originating bees."

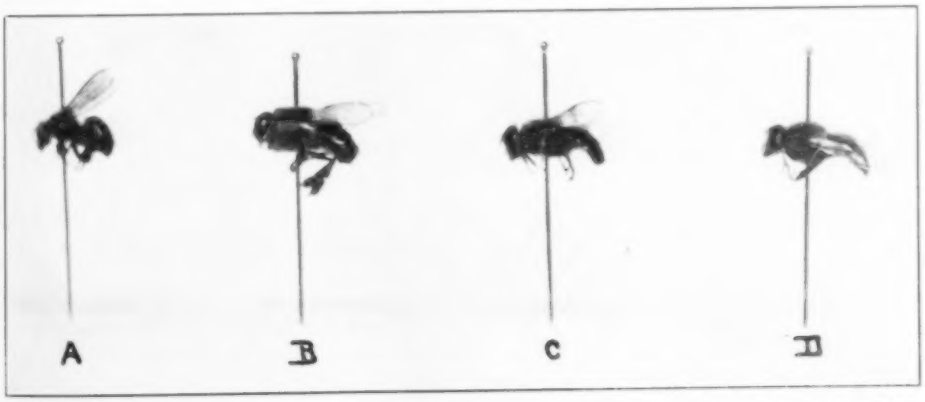
Melanchthon (1497–1560), friend of Luther, considered it a divine provision of nature that bees could be bred from carcasses, and the Italian, Giovanni Rucellai (1475–1525), wrote a poem, "Le Api," containing an account of the Bugonia myth.

Shakespeare in King Henry IV, pt. 2, probably refers to this myth in the following words:

> 'Tis seldom when the bee doth leave her combs
> In the dead carrion.

Moreover, the generation of bees in carcasses was accepted without question by the following seventeenth century naturalists: Aldrovandi, Moufet and Samuel Bochart. Indeed, Moufet believed there were two kinds of animal-bred bees—some bred from lions and others from oxen.

Réaumur was the first to point out that the "bees" which bred in carcasses were flies which closely resembled bees, and Osten-Sacken first identified these bee-like flies as the drone fly, *Eristalis tenax*.



A, WORKER HONEY BEE; *B*, DRONE HONEY BEE; *C*, MALE DRONE FLY; *D*, FEMALE DRONE FLY. NOTE THE RESEMBLANCE OF THE DRONE FLIES TO THE DRONE HONEY BEE.

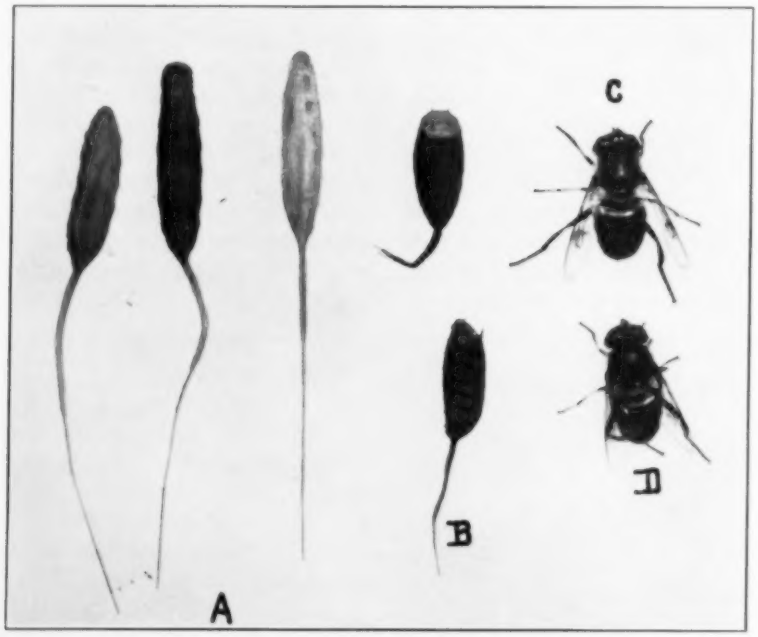
This organism has been well known to naturalists since the sixteenth century. Goedart (1662), Blankaart (1688), Swammerdam (1680), Réaumur (1758) and others described and figured the larva, pupa and imago. Clutius, an apothecary and botanist in Leyden at the end of the sixteenth century, in his book on bees denounced the common error in mistaking bees for *Eristalis tenax*. Vallisnieri (1661–1730), a professor at Padua, and Geoffroy (1762) refer to the fly and its larva. Linnaeus (1766) named the insect and gave the habitat of the larva. Buckton (1895) in his book containing many errors, "The Natural History of *Eristalis tenax*," attempted to present the then known facts about Eristalis. Among others Batelli (1879), Wahl (1899) and Dolley and Farris (1929) have studied the anatomy of the larva. The imago has been used extensively in recent physiological experiments by Professor S. O. Mast, J. L. Wierda and the senior author.

Life History

An outline of the life history of this historic creature is as follows: The imago, or adult fly, deposits its eggs on human feces and in other moist places. The eggs hatch into tiny larvae which grow rapidly into mature larvae. These pupate and remain quiescent for a time and then from the pupae there emerge the active flies.

The Eggs

A single ovum is approximately 0.04 inch in length and floats when placed on the surface of water. We have several times observed oviposition in Eristalis in captivity. Under these conditions the female fly attempts to find a quiet,



—*Photograph by A. R. Apgar*

A, LARVAE OF *Eristalis tenax*; *B*, PUPAE OF *Eristalis tenax*; *C*, FEMALE DRONE FLY; *D*, MALE DRONE FLY. NOTE THE LONG EXTENDED "TAILS" OF THE LARVAE, THE RESPIRATORY HORNS AT THE ANTERIOR END OF THE PUPAE AND THE DIFFERENCE BETWEEN THE HEADS OF THE MALE AND FEMALE FLIES.

damp, dark corner of the cage in which to lay her eggs. Once oviposition has begun it continues usually until hundreds of eggs are laid. It is a remarkable sight to see the female protruding behind her the long writhing ovipositor which deposits an egg, and then after an interval of a few seconds attaches a second egg to the first, and then after another interval attaches a third egg to the others. This process continues for minutes, the female facing away from the growing mass of eggs. She seems oblivious to her surroundings, yet the mass of eggs is neatly arranged, all parallel and attached to one another. About 300 eggs are laid in one mass. Réaumur states, however, that he saw a female ranging her eggs, 20 or more, close to the water on the sides of tanks and the interstices of tubs; he thought the eggs were not dropped into the water. If a number of flies are kept in captivity in a small cage and they are crawling over one another frequently the eggs are strewn about over the bottom of the cage owing to the females being disturbed.

In July and August females frequently enter the laboratory in Woods Hole, Massachusetts, apparently in search of a place in which to lay their eggs, and during these months a large percentage of the females caught in the open will lay eggs in a few hours if they are placed in a cage and supplied with water and cane sugar. On three occasions flies caught in August and September and placed in a small glass vial laid a number of eggs in the vial within a couple of hours before they could be brought to the laboratory.

Buckton maintains probably incorrectly that there are two varieties of eggs: fusiform, which he says he found in females in early spring, and oval or almost globular fertilized ova, which were secured in the late autumn. He believes that the fusiform ova develop into flies which have incomplete reproductive organs and do not oviposit. According to the same author, the eggs are laid twice during the year, in the early spring and in the autumn.

The Larvae

The larvae are remarkable looking creatures with long "tails" through the end of which they breathe. They have received various names: rat-tail maggots (Réaumur), pig worms (Goedart and Blankaart), aquatic intestines (Vallisnieri) and bog house worms (Swammerdam).

The minute larvae, about 0.12 inch in length, emerge from the eggs in from 24 to 48 hours after they are laid, and grow rapidly until they are about an inch in length. For the first few hours after hatching the tiny larvae are highly positive to light, moving toward sources of illumination. They then become highly negative to light, and remain so until they pupate. If the young larvae upon hatching are kept in water they rapidly drown. We have frequently placed eggs upon the surface of water but found it necessary to remove the larvae soon after hatching to more solid material to prevent their drowning.

—*Photograph by A. R. Apgar*

LARVAE OF *ERISTALIS* FLOATING

NOTE THAT ALL BUT ONE OF THE LARVAE HAVE THE TIPS OF THEIR "TAILS" ABOVE THE SURFACE FILM. THE TWO WITH THEIR BODIES FAR BELOW THE SURFACE AND WITH EXTENDED "TAILS" ARE IN THE NORMAL POSITION OF A FEEDING LARVA. THE ONE IN THE LOWER RIGHT HAND CORNER SHOWS THE APPEARANCE OF THE "TAIL" WHEN IT IS RETRACTED.

The food of the larvae is not known, but they are present in large numbers in human excrement in outdoor privies in Woods Hole, Massachusetts. According to Riley, the adult flies were common in 1870 about outhouses in St. Louis. W. W. Smith in 1890 found the larvae in hundreds in "the atrocious stuff" standing in the barrels of a wool-scouring works and in the stagnant waters and drains near Ashburton, New Zealand. Other investigators have recorded the presence of the larvae in the following places: sewers in Vienna and small stagnant pools frequented by cattle (Buckton); dung heaps, cesspools and putrescent waters (Linnaeus); muddy pools (Kirby and Spence); privies, meadows, brooks, moist cow dung (Swammerdam); pulp of rags used in the manufacture of paper (Geoffroy), and "in corrupted waters, depositories of fecal matter and sewage; sometimes also in waters which are limpid and crystalline" (Batelli). Undoubtedly the larvae in the past were found in the putrescent liquid which surrounds the carcasses of dead animals in the last stages of decomposition, but, as Osten-Sacken says, "It is very rare now to come across a carcass and to see *Eristalis tenax* hovering about it."

Réaumur maintains that he has reared the larvae up to the time of pupation on bread crumbs in water. After many unsuccessful attempts to cultivate the larvae we finally devised a perfectly successful and satisfactory method. Some ground "Ralston's whole wheat cereal" thoroughly mixed with water so that it formed a stiff paste was placed in a depression made in moist soil kept in a glass vessel. Eggs placed on the moist cereal hatch into larvae which live in the cereal and mud in perfect condition up to the time of pupation. Under these conditions the following details of their anatomy and behavior can be seen.

Their color is whitish although it has been described as sky gray approaching a little to brown (Swammerdam), dirty yellow (Batelli), bluish white suffused with pink and showing a certain play of iridescent colors compounded of pale bluish and rosy tints (Buckton). Pliny speaks of a "white gem, Eristalis, which appears to blush at inclinations of the light."

Their most striking anatomical feature is the long "tail," which can be very much shortened by one portion being telescoped within the other or can be enormously lengthened. Réaumur says that he forced some larvae to elongate their "tails" to a length of about five inches. As stated above, these "tails" are respiratory in function. The tip of the "tail" is provided with a tuft of hair-like processes which are covered by an oily secretion, according to Dolley and Farris. Keeping this tip above the surface film the larva burrows deeply into the material upon which it is feeding and breathes uninterruptedly through the "tail."

Two large tracheal tubes easily seen through the transparent integument extend from the tip of the "tail" through the body to two spiracles at the anterior end of older larvae. The anterior spiracles are not present in very young larvae.

The integument is very transparent in very young larvae but becomes much more opaque as they grow older. It is very tough and is further strengthened by being studded all over the animal with almost countless tiny spines.

Through the integument, particularly of those individuals which have just emerged from the egg, can be seen plainly at the anterior end of the animal black chitinous bars which are a part of a complex pharyngeal mechanism described well by Miall and others.

According to Batelli, the body of a larva is divided into nine segments

distinguished by furrows with raised edges. On the anterior segment there are present two antennae or sensory papillae. We have been unable to confirm Batelli's statement, "Beneath the antennae are two pigment spots which function as eyes."

The larvae are provided with seven pairs of very short "feet" or prolegs. They have been described by Swammerdam in these words:

> The extremity of each foot is circular and armed like the prolegs of a caterpillar with a large number of fine hooks; the feet can be retracted at pleasure. The hooks are set in a double circle, the circle next to the tip being made up of larger and less numerous hooks than the other.

Another striking anatomical feature is the anal glands—several little pouches which are protruded from the anus at the base of the tail when the animal is quietly feeding. These when extended are wafted to and fro rhythmically but are immediately withdrawn into the larva when it is disturbed. The function of these glands, which are present in larvae of all ages, is not known, but is probably either excretory or respiratory, for they are richly supplied with tracheae.

When a number of larvae are together feeding in a liquid medium and floating at the surface they are frequently gathered in groups with their "tails" much intertwined, "tied into regular knots," according to Buckton. In among these groups are the cast-off skins of the larval stages.

The method of feeding, according to Réaumur, is as follows:

> The larva, living in the midst of abundant food, works over its food much as a pig investigates a pile of refuse with its snout. When enough particles have been scraped loose by the roughened surface of the tuberculatory swellings, they are sucked into the muscular pharynx, where they are removed from the water by a straining apparatus.

Their locomotion has been described by Buckton in these words:

> Though never active in their movements, the larvae, while floating, show a continual constrictive and undulatory motion, which proceeds from the tail to the head, and gives the appearance of creeping like a caterpillar. During this action the front tubercles are alternately protruded and retracted, the water being taken in by gulps and forced backward to the pharynx.

Although they are remarkably tough and resistant to certain unfavorable conditions it is difficult to believe Linnaeus' statement that some larvae which were accidentally introduced through the water into pulp employed in a paper mill survived the pressure of the rollers used in the manufacture of paper. According to Buckton, they will live "many days in clear water free from vegetable or animal matter," in brackish water and in the briny spent-liquors remaining after the curing of bacon. We have repeatedly observed their remarkable resistance to many killing fluids. They will live for several minutes in "Flemming's" containing osmic acid and in other strong fixatives.

The duration of the larval period is in the summer months about two weeks. This period may be lengthened or shortened, however. If the larvae are kept under unfavorable conditions, as in human feces for some reason unsuitable to them, they will leave the material before they are full grown, pupate and hatch into flies much smaller than the normal ones. E. J. Farris in some unpublished experiments kept some larvae living as larvae for over 70 days by placing them in human feces in temperatures a few degrees above freezing. According to Krüger, *Eristalis tenax* spends the winter months in the larval state.

Osten-Sacken maintains that there are a dozen well-authenticated cases where Eristalis larvae have been voided in human excrement. These were no doubt due to the consumption of contaminated water.

The Pupae

When the time for pupation comes the larva leaves the liquid in which it

is living. "Its body becomes encrusted with earthy particles which adhere in consequence of the sticky exudation from the skin," according to Miall. The body and tail become much shortened. The tail and the integument of the body become dark brown and hard.

After twenty-four or thirty-six hours from the time of pupation two pairs of horns appear at the head end. The fore pair is about half the length of the other, and the two pairs often curve towards one another. The fore pair, when diligently observed, are found to be present in the larvae, but they become much more conspicuous at the time of pupation by the smoothing out of the adjacent parts. The large posterior pair appear for the first time shortly after pupation. The horns are really the respiratory organs of the pupa, as is clearly shown by the large tracheal trunks which enter them (Miall).

According to Buckton, the pupae are buried but not deeply in mud. Swammerdam observed the larvae "fixed to the walls of country cottages where they had climbed up to twice a man's height" in order to pupate. Réaumur allowed some larvae to pupate in empty boxes where "they glued themselves to the wood, and if only slightly attached by the hinder part of the body, the transformation was successfully effected: but when the whole of the under side stuck to the wood, the larva perished." We have many times repeated Réaumur's experiment but have never seen larvae destroyed by having the "whole under side stuck to the wood." Pupation is accomplished even though no attachment is made.

The duration of the pupal phase is correctly stated by Miall to be from eight to ten days "in a favorable season." Swammerdam, however, maintains that it lasts about eighteen days. Buckton is probably correct in saying that this period varies with the temperature, but offers no evidence for his statement that it ranges from twelve days to several months. He also states that pupation occurs from the end of June to late October.

Réaumur is probably incorrect, as Miall intimates, in maintaining that before the fly emerges from the puparium it must turn itself completely around in its cocoon.

The Imagoes

The young flies have been repeatedly observed emerging from the puparia. At first they are much lighter in color than the older insects and their wings are much crumpled. Within a few hours, however, the wings have expanded, the normal color has been assumed and the flies are able to fly vigorously.

The resemblance of these perfectly harmless flies to drone honey bees is shown strikingly in the photographs. One does not wonder that they are so frequently confused.

Eristalis tenax also resembles bees in showing somewhat similar color varieties. The males usually have abdomens which are yellower than those of the females, but both sexes vary in the relative proportions of the distribution of the black and yellow pigment on the abdomen.

Moreover, the flies are found feeding with bees upon the nectar and pollen of various flowers, changing readily from one to the other as the season progresses. We have seen them feeding in large numbers upon the blossoms of the common privet, wild mustard, buckwheat, various asters, golden rod, dandelion and occasionally on wild carrot. Near Buffalo, New York, when all other flowers have been killed by frost Eristalis may still be found feeding on dandelion blossoms.

The feces of flies feeding in the open are colored yellow by the numerous pollen grains which have passed through the intestinal tract. The feces have the peculiar odor characteristic of a beehive.

The flies are very common in Woods Hole, Massachusetts, in July, August and September; in Buffalo, New York,

in September, October and the early part of November, and in Baltimore, Maryland, and Ashland, Virginia, in October, when many individuals come into dwellings and laboratories, probably attracted by the higher temperatures within the buildings. According to Banks, Eristalis is common in April, July, September and October near Sea Cliff, Long Island, New York. Buckton states that he has found one female near Haslemere, England, in December and several females as early as March 15.

The time at which the flies become sexually mature is not known. A large percentage of the flies captured in the open in Woods Hole, Massachusetts, in July and August are sexually mature and will deposit their eggs on being taken to the laboratory, as stated above. However, practically none of the flies caught near Buffalo, New York, in late September, October and November will oviposit when brought into the laboratory. Out of hundreds of flies caught at this time only about three oviposited. According to Buckton, the fly he found in December was "an impregnated female, with numerous ova." He also records having found immature females on "May 18, June, August, to October 8," and "impregnated" females from September 12 to late October. He says that sometimes immature forms comprise the larger number of the flies on the wing.

The same author maintains that the female flies hibernate during the winter, crawl out of their hiding places in the spring and soon become actively engaged in oviposition. However, that the adult flies do not hibernate in New York is suggested by the facts that at room temperatures they demand constant access to water in order to live, and that the flies die rapidly when placed in temperatures several degrees above freezing, as found by E. J. Farris in some unpublished work. Opposed to this view is the statement of Buckton that the single female he found in December had been subjected to a temperature of 13 degrees Fahrenheit.

We have made numerous attempts to keep the adult flies living in the laboratory under artificial conditions. In the most successful of these several female flies were kept in splendid condition for four and one half months at ordinary room temperature. They were in a small wire cage. Water was always accessible. Their food consisted of "Ralston's whole wheat cereal" ground fine. This was mixed with fine cane sugar and kept moist. When placed under these conditions the flies of both sexes live well for about two weeks. Then they begin to die and a small percentage of the females only survive for long periods.

Our knowledge of the parasites of Eristalis is slight, as may be expected. According to Buckton, Eristalis is accompanied by its natural parasite, *Tropidopropria conica*, Tab. We have several times observed the ordinary bald-faced hornet, *Vespa maculata*, feeding on the drone flies, and on a few occasions have caught flies bearing one or two mites of an undetermined species. W. H. Bernhoft has discovered that they also harbor a minute intestinal parasite probably closely related to if not identical with the flagellate, *Herpetomonas muscae-domesticae*, found in the intestine of the house fly, *Musca domestica*.

Distribution

The distribution of the drone fly is practically universal. Frauenfeld says: "*Eristalis tenax* is in its ways a remarkable phenomenon, for which there does not seem to exist any other boundaries in time or space (vertically or horizontally) than those which put an end to insect life in general." It occurs

throughout the United States, is abundant in Mexico and Canada and has been reported throughout Europe, in Lapland, China, Japan, southern Italy, Corsica, Malta, Algiers, Gibraltar, Madeira, near Mount Sinai, Canary Islands, Madagascar, New Zealand, Bourbon, Siberia, Northern Persia and in Syria.

The history of Eristalis in the United States is interesting. Its first recorded appearance in this country was in St. Louis in 1870, but it probably did not exist in this locality very long previously. The first specimen seen on the Atlantic coast was found by Osten-Sacken in Cambridge, Massachusetts, on November 5, 1875. The next year several were seen in Newport, Rhode Island, in October and November. In 1877 hundreds were caught in this vicinity. A few years later it was reported from nearly all the states, including California and Washington. Osten-Sacken very properly remarks: "This sudden appearance of *Eristalis tenax* in all parts of the United States, in localities thousands of miles apart, and within a short period of three or four years is a very extraordinary phenomenon and requires an explanation." According to the same author, Eristalis migrated from Japan or eastern Siberia to the North American Pacific coast perhaps long ago, but did not spread across the continent because the necessary conditions for its existence were absent. When, however, man moved from the Pacific to the Atlantic in sufficient numbers Eristalis followed him, breeding in his excrement, and when once the Eastern seaboard was reached it very quickly became abundant and wide-spread.

A somewhat similar thing happened in New Zealand where, according to Osten-Sacken, Eristalis was first noticed in October and November, 1888, but two years later was found to be widely dispersed in large numbers.

THE PHYSICAL BASIS OF INTELLIGENCE

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ONE could say at the very outset that the brain is the physical basis of intelligence and let it go at that, but the inquisitive investigator would yet wonder why some individuals are smart and others dull. Doubtless all the different types of mankind now classed under the name *Homo sapiens* belong there biologically, but are downright mental disgraces to the title. Many outstanding individuals would offer no objections to the compliment, sapient, and yet it does seem a far-fetched use of the term to apply it to a Tasmanian or a microcephalic idiot. What are fools, morons, seers and sages? The every-day speech of man has recognized the differences in mental ability, and a crude but working classification has sprung into common use which in a vague sort of way takes cognizance of brains in the proper cataloguing of the mentality of its individuals.

What differentiates the fool from the sage? Both have brains. One has more than the other, one has a greater mass, a greater weight and is therefore more intelligent. We shall not stop here to define intelligence, but hurry on as if we knew and in the determination of its physical basis offer a definition. The brains of normal individuals do vary in size and weight, from small brains to very large ones. There are limits below or above which the variation can not go without encroaching upon the abnormal. Abnormal mental types have brains of abnormal size which range from the microcephalic idiots at the bottom to abnormally enlarged brains at the top, but even between these two extremes, where mental ability must be classed as normal or borderline, there is wide variation.

It has been generally believed that large brains reside in large heads, and some years ago a series of scientific investigations was undertaken to determine the correlation between head size, brain size and intelligence. No physiological correlation was found, and the association between head size and intelligence was relegated to the discard, perhaps too hastily. Even if there is no recognized correlation between head size and intelligence in the normal in a physiological sense there is some definite association in the pathological sense, for it is now well established that heads abnormally large or small have something wrong with the brain. Abnormal mentalities reside in abnormal heads, which expresses itself in size or shape of the cranium, but no particular size or shape can be said to be characteristic of any given condition. The size and shape of the head have little significance in determining the intelligence, but the modern neurologist makes use of it in establishing the departure from the normal, in which case it is a clue of valuable neurological significance.

The size of the head, and that usually is an index to the size of the brain, can not be relied upon as a measure of intelligence; its use in the abnormal is mainly for diagnostic purposes. However true the above may be, there are many investigators who are strongly convinced that the size of the brain is a general index to the intelligence of the individual and in support of that contention have cited the fact that the brains of our intellectual leaders have been and are above the average. The fact that some great men have small brains lessens the validity of this contention.

The absolute size of the brain and the

methods of its calculation from measurements of the head is a very important adjunct to the equipage of neurologists engaged in determining the degree of mental defectiveness and the limits of departure which can be classed as normal, but measurements in themselves are only an indication that something might be wrong, so it became necessary to seek elsewhere for a reliable physical basis of intelligence. The deeper search revealed the convolutions of the cortex, and at once the relation of the fissured and convoluted cerebrum to intelligence in general was attacked. The number, depth and length of the convolutions were investigated with the hope of discovering some correlation between the mental ability of the individual and cortical configuration. There can be no doubt that convoluted brains do not arise in mammals of low order, but gradually increase in complexity until the genus homo is reached. It is also quite true that the convolutions greatly increase the surface area of the neopallium. Inasmuch as the gray matter is continuous over the whole surface area, into the depths of the gullies and on the tops of the ridges too, it follows that the more numerous the fissures and the deeper the gullies the more extensive the area of the gray substance upon which it has been thought intelligence depends.

The convolutions, their extent, their configuration and depth have been carefully studied and compared, with little success, so far as correlating the mental ability of the individual with the surface area is concerned. There was found to be a broad correlation physiologically, but so broad and so general that it was inaccurate for scientific measurements or determinations. The weakness was most pronounced in some cases of mental defectives. As a general thing the surface area of the normal individual is greater than that in low mental types, but in some cases the surface area of the dement is even higher than the normal.

It does appear, then, that neither the size nor the surface area of brain can be used as an accurate and infallible guide to the determination of the mentality of the individual, and brains so classified can not be indisputably placed in categories of high or low intelligence. It then becomes necessary to seek elsewhere for the physical basis, remembering, however, that size, weight and surface area constitute important aids that can not be ignored, for it is well established that leaders in all lines of intellectual endeavor have brains and bodies that stand out as heavier, larger and with a greater surface area than mediocre or inferior individuals. After all, the contention that great men have greater and heavier brains is true to a limited degree. A much more reliable basis than either mass or surface area is the thickness of certain layers of the cortex, which must be explained to be understood.

If a section through any part of the cerebral hemisphere is examined carefully with the unaided eye it will be seen to be composed of an outer layer of gray and an inner layer of white substance. The outer layer is the cortex and the inner is the medulla. The outer layer is made of cell bodies mostly, while the medulla or white substance is composed almost exclusively of fibers. The cortex is much thinner and is made up of alternating strata of lighter and darker materials which can be recognized upon close inspection with a hand lens. From without inwards these layers of the cortex are: (1) superficial band; (2) a superradiary band; (3) an outer band of Baillarger; (4) an inner band of Baillarger. These bands can be recognized in the fresh brain and are not to be confused with the layers that can be recognized when the brain is fixed and stained and examined under the microscope. The histological examination shows that there are five layers, the first one being a superficial fiber lamina, which is made up of nerve fibrils, neuroglia (support-

ing) cells and tangential fibers that interlace in a varicose network and yet maintain a general parallel course with the surface. The second layer is composed of pyramidal cells of large, medium and small size, whose axones run inward, that is, out-bound impulses course over them. The third layer is a granular cell layer formed of closely packed Golgi Type II cells. The fourth is an inner fiber lamina and is made up of medullated axones and an occasional large pyramidal cell. The fifth is a polymorphic cell layer and consists largely of polymorphic neurons. The only correspondence between these histological layers and those of the fresh brain is this: the outer line of Baillarger agrees closely with the middle or granular layer, while the inner band of Baillarger lies at the same level as the inner fiber lamina.

Watson proposes a simplified terminology for the layers of the cortex and reduces the number from five to three. He includes the two outer layers, outer fiber lamina and pyramidal cell layer, in his supragranular cortex. The middle layer he would call the granular cortex, and the two inner layers, inner fiber lamina (layer of axones) and the polymorphic cell lamina, he would include in the infragranular cortex. From the developmental and functional points of view this simplification is an excellent one and takes in all that is essential in the determination of mental reactions. From without inwards they are, supragranular, granular and infragranular cortex.

There is some very interesting information in the order in which these layers arise. During the first three months of gestation there takes place in the developing brain a proliferation of undifferentiated neuroblasts, that is, the cells of the brain multiply only. But during the fourth month, usually along toward its close, a change takes place in this unilayered condition. On the external surface there differentiates a fibrous lamina so that there are now two layers, the fiber lamina and the neuroblastic layer. The fiber layer undergoes no significant change from the fourth month on to maturity; it merely thickens to about double its depth. The neuroblastic layer gives rise to the polymorphic cells in the fifth month, by the differentiation of cells from the internal surface. There are now three layers, the outer fiber lamina and the internal polymorphic cells while the undifferentiated neuroblasts lie between the two. During the sixth fetal month this layer of neuroblasts produces the granular layer and the pyramidal cell layer, the chief part of the supragranular cortex. All five histological layers are formed by the end of the sixth month but none of them are as thick as they will be at birth. It is very significant too that all five layers do not thicken at the same rate.

The infragranular cortex arises first, fourth month, and by the end of the sixth month has attained 75 per cent. of its total adult thickness; by six weeks after birth it has reached 82 per cent. of its total adult thickness, and shortly after the onset of the prepubescent period it reaches its total adult thickness. The infragranular cortex is made of axones in the inner fiber lamina and the polymorphic cells, so that it has a neuronic part and a fibrous part. The incoming impulses from the vegetative organs come to this infragranular cortex over the axones of the inner fiber lamina to the granular cortex and out to the vegetative organs by way of the polymorphic cells. Inasmuch as these outgoing impulses operate the sexual and vegetative functions it is only natural that they should be formed early and be in working order at birth. The polymorphic layer can not function without the inner fiber lamina, so it is formed at the same time, while the very important granular cortex arises next to the polymorphic layer.

The granular cortex receives and stores the incoming impulses destined for the superimposed layer, hence it does not reach its total thickness as soon as the infragranular cortex. At birth the granular cortex is 75 per cent. of its total adult thickness; at the sixth fetal month it was 50 per cent. The mechanism for operating the lower animal functions is this: inbound impulses traverse the afferent axones to the inner fiber lamina to the granular cortex, where they are received, stored and discharged over the cells of the polymorphic layer to the viscera. The three layers, inner fiber lamina, polymorphic cell lamina and granular cortex, are parts of the primitive pallium and in some mammals constitute the entire layers of the cortex.

The pyramidal cell layer, the most important component of the supragranular cortex, is merely recognized at the sixth month. At birth it has increased to 25 per cent. of its total adult thickness and at six weeks after birth has increased to 50 per cent. During the next six weeks it increases about 10 per cent., so that it attains 60 per cent. of its total adult depth by the middle of the first year. This layer is concerned with those types of reactions which are commonly referred to as intelligent, and though 60 per cent. of the total number of cells are present six months after birth they are not all working because the process of myelinization has not been completed on all of them and no axone is in perfect working order until it is medullated. In the supragranular cortex reside the highest cortical functions such as control, inhibition, reason and educability. The layer being thin and imperfectly working at birth has very little influence on the infragranular cortex, and the baby shows no control over or inhibition of the vegetative apparatus. The supragranular cortex does not increase at the same rate in all individuals nor does it grow at the same rate in both sexes.

Prior to 1920 there were no studies available on the growth of the brain after birth. In that year Berry and Porteus reported after a study of ten thousand brains that there were definite periods of growth in the human brain from birth to maturity. The first period, the prepubescent, comprises the first eleven years of the child's life. During those years the brain of girls reaches 91.3 per cent. of its total expected volume, while the brain of boys reaches 88.8 per cent. of its adult size.

During the twelfth year there is a decided and definite slowing up in the rate of growth, and in this resting period the brain increases only .6 per cent. in both sexes. This resting phase occurs just before the onset of puberty, and it is quite significant that high-grade aments show abnormal mental traits at this period.

The third period includes the thirteenth and fourteenth years, the period of puberty, during which the brain of girls increases 3.4 per cent. and that of boys 2.2 per cent.

The fourth is the postpubescent period, during which the brain of the female increases 4.7 per cent. and that of the male 8.4 per cent. It would appear that the brain of females reaches maturity slightly in advance of the males. It has been shown by Bolton that the postpubescent increment to the cortex is the one that most often fails to be added, which shows up more or less decidedly in the mental reaction of the individual just as the sexual mechanism matures. In some cases even the second and third increments are not added, and a mental defective of the ament grade results. When the postpubescent addition is not made the controlling and inhibiting impulses are either absent or feebly developed, and the behavior of the individual approaches the animal level. The infragranular cortex is rarely diminished but the superimposed layer is, and that may result in a small head. The

measurement of the head and the calculation of the cubic volume of the brain therefrom is not at all an accurate determination of the mentality, but when the deviation is so far from the normal that it is startling then do we find some type of mental defective.

The high cubic capacity of the brain may be due to two totally different things. The size of the brain depends upon the number of cells composing it, but not all the cells of the brain are neurons; some are supporting cells. Large brains made up mostly of neurons produce seers and sages, but large brains made up of a superabundance of supporting tissue produce the big heads of inferior and mediocre ability. Large brains of inferior type are caused by the conversion of too much potential nerve stuff into supporting stuff during the early differentiation of the layers of the cortex.

One may assume that if the cortex stops its growth or is stopped by some cause known or unknown the normal micrometric depth is not reached and the mental reactions suffer because of it. Some internal secretions may stimulate the cortex to renewed growth as the thyroid does in cretinism, but for other causes of arrested or delayed growth of the supragranular cortex there is yet no stimulating substance known. In the idiot the thickness of the supragranular cortex is but 25 per cent., or less, of the normal value. In the imbecile the thickness is 25 per cent., and in some types of dements the depth may be no more than 60 per cent. of what it should be in the normal. In all forms of mental backwardness and lack of intelligent response and adjustment there occurs a retardation in the growth of the supragranular cortex, or a direct loss of cells from the pyramidal layer. In the imbecile and idiot the infragranular cortex is of normal thickness, as is also the granular cortex, so that the vegetative functions have the same efficient mechanism and the intensity of the impulses arising therefrom is in no way diminished, but the efficient mechanism for the control of these functions is lacking. The ability to reason, make mental adjustments, to be educated, is hampered or absent, because the physical basis for such high mental functions is lacking.

There is a limit to the size of the brain that permits of intelligence. If the brain is smaller than 930 grams there is no possibility of having any human intelligence, but this is a far less important consideration than the relative thickness of the layers of the cortex. Mankind did not acquire the laminated cortex at a single bound, but gradually through the progress of the ages it was added little by little. Probably the ability to become so endowed with a great cortex was bequeathed to man by his protohominid ancestors. In the cortex of mammals the same layers are found as in mankind, but the relative thicknesses are vastly different. The infragranular cortex is of about equal depth or the variation is slight enough to be ignored. The granular cortex is about equal. The supragranular cortex, however, is almost entirely absent, and even in cases where it is present it is about one fiftieth as thick as in man. In this fact lies the utter absence of control and inhibition that the animals manifest in the matters of food and sex.

The explanation of cortical function is to be found in the connection of incoming impulses with the outgoing ones and the passage of the impulses through the several layers of the cortex. The interconnecting neurons are relatively short and are interposed between the termination of the afferent axones and the origins of the efferent fibers, where they occupy the relative position of a rheostat in an electric circuit. In the layers of these internuncial neurons the incoming impulses are arrested, stored up, lost or discharged. It is this function of arresting, sorting, modifying, storing, dissi-

pating or discharging impulses that is responsible for the phenomena collectively called "mind." Impulses that are brought to the cortex by the afferent axones reach the granular layer first; here the manifold duty of sorting and storing takes place, involving a limited number of neurons. Even though the actual number of neurons is large it is limited, and the possible connections that can be made depend upon the actual number of neurons present in the cortical layers. Not all incoming impulses arouse a "set up"; some are lost, others stored; but some induce a complete circuit and are discharged over the appropriate, or inappropriate, pathways. Thus does this function, mind, take no cognizance of intelligence. If something interferes with the development and operation of the internuncial neurons a corresponding impairment of the mind results. The development may go on to normal and some disease or poison destroy many of them so that interconnecting function is hindered, a departure from the normal takes place which may go so far as to constitute insanity. Mind is function, the operation of the machine, the interconnections formed between the internuncial neurons for the transmission of impulses through the cortical layers.

But what of intelligence? At first blush it may appear that intelligence has no place in this scheme. The mechanical operation of the units is enough seemingly to determine the reactions, about which there is nothing mysterious even though the operation is exceedingly complex. Intelligence depends upon factors which are more subtle, and two in number. The first is the number and nature of the neurons and the second is the number and nature of incoming receptor impulses. Mental reactions, which can be measured, should not be confused with intelligence, which can not be measured. They are not distinctly different phases of the same thing. It can very readily be seen that if there are many neurons the incoming impulses will have a greater variety of pathways over which to travel and there will be a greater chance for them to be stored; there will be a greater number of possible selections. Perhaps the storage depends upon the number and quality of the incoming impulses and the number of repetitions, and this in turn depends upon the number of neurons in the supragranular cortex capable of entering into the reaction. Impulses are continuously shot into the cortex where certain combinations of the neuronic arcs are made, and the more neurons there are present the more appropriate will the reaction be; the more appropriate the reaction the more intelligent the response. Mind depends upon the incoming impulses which are always more numerous than the outgoing ones, and the intelligence of the reaction is quite dependent upon the possible "hook ups" that can be made in the circuit. Mind is the operation of the mechanism; intelligence is the use of the number and kind of units involved in this mechanism. The units are the neurons in the supragranular cortex, influenced by the number and kind of impulses that come in over the receptor neurons. It is at once evident that the reaction which any individual makes to incoming impulses will be largely, if not entirely, governed by the arcs that can be established between the incoming and outgoing neurons. The previous experience of the organism, the training and the use to which his neurons are put, have so much to do with his intelligence. It must not be lost sight of, however, that if the number of neurons is small the chances of being able to make an intelligent "hook up" are reduced to a minimum, while if the number is large the chances are greater, and two individuals with the same training can not profit by it to the same extent.

In the evolution of the vertebrate

brain nature has added a series of coordinating brains on top of the old. At the top in the first stage lies the midbrain; in the next stage thalamo-striate brain was added, and on top of that was placed the infragranular cortex. Finally in man the supragranular cortex was added. Nothing has been taken away; each additional part was intimately connected with the old, forever increasing the complexity, ever increasing the number of neurons at the top and the possible number of combinations between them. This soon becomes so complex that the experimenter can not predict beforehand just what pathway the outgoing impulse will take, and is able at best to form only a very general prediction. The destination of the outbound impulse impels his behavior, in the sense of the neurologist, not the psychologist. This can not be foretold definitely because of the complexity of the neuronic arcs and the numerous possible routes over which the outbound impulse may be dispatched.

Psychic reflexes, emotional reflexes, are delayed responses, instead of immediate, to the incoming receptor impulses which reach the supragranular cortex, the most recently added part of the pallium. Here are the most numerous and most complex neuronic arcs, the switching system for the tracks over which the outwardly dispatched impulses are to travel. Psychology has concerned itself with these outbound tracks which are, after all, only part of the entire process and can claim but a small province in the great empire of brain physiology.

Probably the higher reflexes are essentially similar to simple ones but are more involved because of the concatenated series of neurons and neuronic arcs over which the impulses must go before they occasion a display, the collective phenomena of which constitute mind. There can be no mind without neurons, and there can be no intelligence without neurons and incoming impulses. Upon the neurons, not only their nature and number but also their use, does intelligence depend.

The physical basis of intelligence, then, is the neuron, especially those neurons now residing in the supragranular cortex. The number of neurons in that layer determines its thickness. If few are ever developed the individual is an ament; if more are formed the individual may become a high-grade ament or moron. If the postpubescent increment has been partly or completely added and then is destroyed the individual becomes a dement. If the normal thickness is attained without subsequent destruction the individual is normal in his mental reactions. The normal varies in thickness, but within definite limits. If the layer is very thick, which means that many cells are present, the brain will be large and the layers thick, and the individual will be a multineuronic genius. The brain may be just as large in another individual but the cells not neurons but neuroglia, in which case the brain will be that of the big-headed type of inferior or low intelligence. If the thickness is average the intelligence can not rise above that level.

Fools, morons, seers and sages are such by virtue of the neurons in the supragranular cortex and the use to which these neurons are put. The brain must receive before it can put out, and of course the more there is to receive the more there can be accomplished. It is the function of correlation, integration, association and storage of impulses by the internuncial neurons and their use which occasion intelligent responses and the more of all this the individual is called upon to do the more attuned will his life be to the world into which he has been cast.

THE SCOURGE OF YELLOW FEVER: ITS PAST AND PRESENT

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THE present generations dwelling in the Southern states, in Cuba, Panama, Mexico and Central America, living in the midst of excellent health control and thus unafraid of the great plagues that formerly overran certain portions of the earth, give little heed to the possibility of that terrible epidemic disease, yellow fever, which for centuries (1650 to 1900) ravaged the regions above mentioned, attacking all classes of society, causing the death of thousands, paralyzing the activities of man, so long as it remained in a given locality, leaving behind, after its disappearance, a trail of woe and sorrow, of poverty and distress. In some parts of the Gulf Coast (Gulf of Mexico) where the disease appeared periodically, as in Texas, Louisiana, Mississippi, Alabama and Florida, the people were barely recovering from a summer's visitation before a new invasion or a renewal of the infection took place, so that its material progress was in this way seriously curtailed, not to mention the consequent emigration of a fair part of the population into other states, in an effort to escape the threatened calamity during the hot months of the year.

One of the features that stamped yellow fever as a particularly fearful disease was its spectacular appearance, the sudden onset and quick death of those attacked, usually within a week or ten days after their first symptoms. Other diseases, not less dreadful, bubonic plague, typhoid fever, typhus, are more insidious in their presentation, last longer in their course, and the families of those afflicted enjoy a certain period of hope that makes the disillusionment less painful. But at certain times in some of the epidemics of yellow fever the people became panic-stricken, many fled as soon as they could, others even abandoned their dying relatives, and there were many occasions where a great exodus of the population made it impossible to bury all the dead. Many communities of the surrounding country, where yellow fever had made its appearance, established the so-called "detention camps" to prevent free emigration from the stricken towns, and the old shotgun quarantine when deputies carrying firearms readily shot at any one trying to cross sanitary cordons without a previous thorough examination and protracted detention to insure his enjoying absolute freedom from infection. In the "detention camps" of the South, many died from privation and exposure, as they were seldom prepared to care for the improvident emigrants who frequently at the same time congregated there. The period of observation in some of these human (or inhuman) corrals varied according to the opinion of the local authorities or of the people from two weeks to the preordained forty days. And then when a case of yellow fever developed in any of them, as was often the case, it became necessary to move camp, and, as it was then customary, everything not absolutely necessary to life or that had had any contact with the case was set afire for its purification, the only means of disinfection then deemed effective.

It seems to us to-day like the memory of a nightmare in the course of our lives, but I have not exaggerated or overdrawn the picture of those awful days. In fact the tragedy was greater than I have depicted, for along with abandoned homes went robbery and pillage, with the loss

of parents, the hunger of children, the despoiling of young girls, the abuse of brute force, and with the fear of impending death came the relaxation of moral ties and restrictions (though one would think it would be otherwise), this being a human phenomenon that has been witnessed in mostly all great calamities of any duration, crime increasing in all periods of general distress. Along with this, however, as also observed in all great conflagrations, acts of self-denial and heroic altruism were not lacking amongst the inhabitants of the afflicted communities.

We have been taught to think that yellow fever was a disease of the South, of the tropical or subtropical regions of America; but when we come to delve into the musty pages of history, we find that, although presenting itself before civilized peoples for the first time in the Caribbean Islands, some of its subsequent incursions extended far North, to Boston, even, to Philadelphia only seventeen years after its settlement, when it consisted mainly "of a few hundred families from the mother country clustered together" forming a small country town.

There is a great deal in the vast literature accumulated that bears upon the history of yellow fever; in perusing the many works relating to this part of the subject we are at once struck by the fact that at no time has the disease appeared or been suspected to appear in the tropical countries of the Orient; India, Indo-China, Southern China, Japan, the Philippine Islands seem to have escaped or have never had any epidemic disease that might be mistaken for yellow fever. This point is one that, in my opinion, deserves consideration and study, for inasmuch as the necessary climatic (heat) and entomologic (mosquitoes) conditions exist there, and the disease has not as yet been extirpated (and it will be very difficult to do so) from the other side of the Atlantic (West Africa), the danger of invasion of the Oriental countries will subsist, in a measure that is not generally appreciated. And worse still, the danger will be greater as exploration and commerce, by establishing new and more convenient (more rapid) means of communication between the countries surrounding the Great Desert and the western coast of the African continent, increase the chances of its transportation towards the Mediterranean and further east.

In looking for the origin of our American epidemics there is much matter that must be considered irrelevant; for instance, we can not believe that it was yellow fever that affected the tribes of New England, as reported, before the arrival of the Puritans, in which, according to tradition, they bled at the nose and became yellow, dying in such proportions that from 30,000 they dwindled to barely 300, for the disease thus vaguely described raged during the winter months; no more worthy of credit are the narratives extracted from Maya Indian records, carefully studied by Finlay and others, which might have been epidemics of malaria as well as yellow fever. A painstaking study of many documents dealing with this matter has led me to believe that yellow fever came to America from Africa, and that the western coast of that continent has been the natural abode of the disease. We have evidence of the comparatively mild character in which it appears among the Negroes, and only lately (from 1910), since it attacks the white inhabitants, has the question been properly investigated. From Africa it came to us with the slave trade, perhaps first to Barbadoes, as the records seem to show, spreading thence among the West Indies, and no doubt being periodically reinforced by new material brought with the slavers of the early days. My inference that it came from Africa is in relation with other diseases which also came with the Negro slaves but failed to spread in America because the "spreader" was not here, the means of

propagation was lacking; there are persons living in Cuba, for example, who remember having seen cases of sleeping sickness (trypanosomiasis) among the Negro slaves, who went on and died of the disease, here known as *modorra* or laziness. In some instances it was believed that the slaves assumed that condition purposely so as to die and "thus go back to their country." The absence of the tsetse fly precluded any possibility of the disease spreading in America.

And so we come to the first authentic epidemic among white people, in the island of Barbadoes, in 1647. In speaking of the disease during the following year, Du Tertre[1] says that "in the course of eighteen months it carried off one third of the inhabitants." All the islands bordering upon the Caribbean Sea, as well as the mainland (Panama, Darien), subsequently suffered epidemics of yellow fever, while in some of them the disease established itself definitely, raging during the summer, less active in winter, but continually demanding a fair number of victims from the population. The lines of trade, then gradually developing, brought the infection to Charleston, South Carolina, and the same year to Philadelphia, as noted above. The fever went further; during 1694 the city of Rochefort in France was invaded; two years before, a naval expedition intended for Canada, after visiting the island of Martinique, caught the disease and, arriving in Boston, June 17, reported having buried at sea 1,300 out of 2,100 sailors and 1,800 out of 2,400 soldiers! The "distemper" from the fleet extended to the city, causing the exodus of many families to the country until the infection disappeared.

The early nineteenth century was rich in epidemics of yellow fever, not only in America but in Europe as well. In 1804 it went as far as Leghorn, Italy, Lisbon, Portugal, and twenty-five cities of Spain were invaded. It also reappeared in New York, Philadelphia, New Haven and Providence.

In the United States, the South continued to suffer periodically until 1905 when the last epidemic occurred in New Orleans. The same happened in the Caribbean countries, in Brazil, Ecuador, Mexico, Cuba, etc. I refrain from setting here the calculated number of lives lost during the space of years herein recorded; aside from the fact that it would be inaccurate, as no records can be examined from the countries that suffered the most, the figures that I could guarantee as true ones are frightfully appalling.

This disease was particularly dreadful to the people because of the rapidity of its evolution. A person enjoying apparently perfect health will suddenly feel sick; headache, severe unto a sense of bursting, often accompanied by a slight chill; soon severe bone aches will rack the individual and the fever will make itself felt; at the end of 24 or 48 hours all symptoms may subside or suddenly grow worse, with uncontrollable vomiting, first of gastric juice or food, later streaked with blood or containing coffee ground matter. The kidneys become congested and later more seriously affected, sometimes to the extent of complete suppression of all function, thus rapidly leading to death. If there are no complications, cases are usually settled, one way or another, within six or eight days; if the heart, kidneys, lungs or liver were previously diseased, yellow fever often takes its toll in very short order; or if the attack is mild, even under those circumstances the convalescence is prolonged many days, sometimes leaving permanent lesions that in the end will shorten life. Thus in such a short period of time, under great pain, delirium, convulsions, perhaps, patients die in greater proportions than in most other infectious diseases. Some cases see their end in 24 to 48 hours, all the

[1] "Hist. Gen. des Antilles," p. 422.

symptoms piling up within that time. A constant sign of the disease and one that gives it its most common appellation is jaundice in greater or lesser degree and always more evident after death, when the glow of the blood circulation disappears from under the skin. Of course, there are many various types of the disease, which would be entirely out of place to describe here; we have only intended to show that its course is usually short, the symptoms prominent, severe and distressing and that death is too often all too rapid. When patients finally recover they remain immune for the rest of their lives.

As it may be readily surmised, during all those years ef repeated epidemics in various part of the Americas, the medical profession did not remain inactive; on the contrary, the most eminent clinicians were deeply concerned in the investigation of the causes that produced such dire results in their respective localities, and the theories advanced and the explanations presented filled the pages of both the scientific and the lay press. The old theory of miasms or effluviae was early thrashed over; the emanations from putrefying material, coffee that rotted at the docks in Philadelphia, was accused: it had been noted that the infection extended along the wharves, following the river shores; the foul sewers, where these existed, were also blamed for the "pestilence," "exhaling streams of unwholesome and poisonous gases." One thing was generally accepted by all who studied the epidemics —the influence of altitude. It was soon observed that the fever often invaded towns along the seashore or in flat lands and persistently kept away from even slight elevations, or when separated a comparatively short distance from the sea. This notwithstanding, during severe epidemics and exceptionally it has been known to extend beyond its usual limits to towns in the higher altitudes.

Beyond these facts, repeatedly corroborated, no real advance was made towards clearing up the epidemiology (manner of its spread) of yellow fever. In the last quarter of the last century, the science of bacteriology, thanks to the remarkable work of such men as Pasteur, Koch, Behring, etc., acquired great importance and worth in the hands of investigators the world over: the germs of the most dreaded infectious diseases were discovered (cholera, anthrax, tuberculosis, leprosy, diphtheria, typhoid, plague, etc.), and so there was great stimulus brought upon research of yellow fever. In Mexico, Brazil, Uruguay, Cuba and Ecuador, men thought they had found the cause of this disease, only to suffer disappointment in the end, for the germ or parasite of yellow fever has not been found as yet. Some of the claims were very hard to prove false, as the authors were men of high standing and profound scientific knowledge and training; such was the case with Sanarelli, an Italian bacteriologist working in South America, and with Noguchi, a Japanese savant, who mistook the cause of another disease (infectious jaundice) for that of yellow fever. Noguchi went to the extent of preparing a vaccine to prevent yellow fever and also a serum to cure the disease, both things, as we know now, perfectly useless. And it seemed an irony of fate, under the circumstances, that this great bacteriologist should lose his life through an attack of yellow fever in West Africa just as it was proved that his organism (his discovery) had nothing to do with his infection.

So far, although we know where and when the germ or parasite of yellow fever may be found, it has never been brought to light and therefore its appearance and its nature are still unknown. We know it is filterable, that it passes through the finest porcelain filters, and that it circulates in the blood until the third day of the disease and then can be detected only in the tissues

of some of the internal organs, so that when that blood or said tissue is inoculated into susceptible persons or monkeys they suffer an attack of the disease.

Until 1900 the quest for the cause of yellow fever had been entirely fruitless: the manner of its spreading was only conjectured and its prevention and treatment, therefore, a matter of hit or miss, in the hands of the most expert.

When the United States government took charge of the political administration of Cuba and Porto Rico, it soon established modern sanitary measures that brought down both the sick and the death rate, particularly in connection with the infectious diseases. Yellow fever, however, seemed to have been not at all affected, and the disease continued to collect its quota from the foreign population, Americans and Spaniards mainly, apparently in the same proportion as before. Here was an epidemic disease which, contrary to all expectations, refused to be put down by the same means that were successful as regards the other epidemics of beriberi, dysentery, typhoid fever, smallpox, etc. It was evident that a further study of its causes and the manner of its infection was necessary.

Major William Crawford Gorgas, who was later to acquire world-wide renown through his outstanding work in Panama, was the health officer of Havana, and General Leonard Wood (an army surgeon before the Spanish American War) was the military governor of the island.

The surgeon general of the army was George M. Sternberg, one of the most distinguished pioneers in the science of bacteriology, and he naturally gave much importance to the laboratory investigation of all diseases; he therefore appointed a board for the purpose of studying the infectious diseases of Cuba, with special reference to yellow fever. This board was composed of Major Walter Reed and Surgeons James Carroll, Jesse W. Lazear and the writer.

Work was begun on June 25, 1900, by distributing the necessary details among the several members, one man doing the post mortems of those who died, the other the bacteriologic work, planting of tissues and secretions of cases in an effort to isolate the germ, and the other the preservation and study of the organs obtained by autopsy. Major Reed, being the chairman, had no special work assigned to him; in fact, soon after the board convened, he had to leave for Washington so as to finish up the report upon "Typhoid Fever in the Army" which was being prepared in collaboration with Drs. Shakespear and Vaughn.

In the last days of June an epidemic of yellow fever developed in a city in the interior of the island, and in the first days of July, by autopsy of some cases supposed to have suffered from pernicious malarial fever, I discovered an epidemic of yellow fever among the soldiers of the garrison. It was at this epidemic that Major Reed and I first discussed the possibility of mosquito transmission, and upon our return to Havana it was unanimously decided to give the matter a thorough sifting.

The idea of the mosquito as a factor in the propagation of yellow fever was not new to us. Many years before (1699) Dr. Rush, of Philadelphia, remarked upon the excessive numbers of those insects at the time of the pestilence in that city; in 1848, Dr. Nott, of Mobile, Alabama, suggested that the yellow fever poison was disseminated by means of some insect "that remained very close to the ground." In 1854 Dr. L. D. Beauperthuy, French physician residing in Venezuela, published his ideas regarding yellow fever, holding that the disease was caused by the bites of mosquitoes; that it was not found where mosquitoes were scarce or absent; that it was produced by the same agents that brought about the intermittent

fevers; that the striped-legged mosquito was a domestic variety, etc. Believing very justly that yellow fever is not contagious, he could not think of man-to-man transmission; this was left to Dr. Carlos J. Finlay, a Cuban of English origin, who in 1881 gave his opinion that yellow fever was spread by the bites of mosquitoes "directly contaminated by stinging a yellow fever patient (or perhaps by contact with or feeding from his discharges)." Most unfortunately for his greater glory, Dr. Finlay was never able to convince any of his contemporaries, he could never produce a single experimental case of yellow fever, and although some of his cases at some time or other had abnormal temperatures, we know now, positively, that not one of those inoculated by him could have developed, as a result, an unquestionable attack of yellow fever. This failure in his part tended to discredit his theory which *in substance* was the correct one, though not exactly as he originally enunciated it.

Dr. Finlay was consulted when we decided to investigate his theories—a fact that naturally gratified him greatly. He gave us some mosquito eggs which subsequently hatched, the insects finally dying without causing any harm; he told us of his ingenious methods of infecting the insects, which we followed for some time and subsequently abandoned for better ones.

In many previous writings I have gone extensively into the detailed work of the Army Board and I shall therefore only briefly sketch it here, since we are principally interested in the results.

Upon obtaining a sufficient number of the mosquitoes, the three working members of the board (Carroll, Agramonte and Lazear) devoted their time to breeding them and infecting them by feeding them upon yellow fever cases at the hospital, later applying them to themselves and to whoever was willing to run the risk of getting the disease in that way, then considered remotely possible only. One day Carroll was so unlucky as to let an infected mosquito bite him, inadvertently, thereby developing the first case of experimental yellow fever. Soon after, in fact, during Carroll's illness, the same mosquito and others were applied to a soldier who consented to the operation, and he later developed an attack of yellow fever, thus proving beyond a doubt the mosquito transmission of the disease. While Major Reed, still in Washington, was preparing to join us, Dr. Lazear became infected at the yellow fever hospital and died on September 25, 1900. The sacrifice of his promising life was a serious blow to the board, but as soon as Carroll completely recovered, Major Reed having returned from the United States, the experiments were renewed at a small camp (called Camp Lazear) established for the purpose.

Eighteen experimental cases were produced with mosquitoes, by blood injection and by the use of filtrated serum from yellow fever patients, *without a single death*.

Besides this, by having several men living for 20 days in contact with soiled linen, clothes and bedding from yellow fever cases, it was shown that the disease is not produced that way.

The names of John R. Kissinger, John J. Moran and Dr. Robert P. Cooke deserve special mention, inasmuch as they offered themselves as subjects for the experiments, without remuneration whatsoever.

As a result of its work, the Army Board made the following discoveries:

1. Yellow fever is transmitted in nature solely by the bites of mosquitoes; soiled articles (called fomites) being inoffensive.
2. Stegomyia (now Aedes) mosquitoes become infected when they bite a yellow fever patient within the first three days of the disease and not later.
3. Mosquitoes can transmit the disease only after 10 to 12 days of feeding from a yel-

low fever patient and not before, retaining the power to infect during the rest of their lives.

4. The period of incubation (from the mosquito's bite to the first symptom of the disease) does not extend beyond six days.
5. The germ of the disease, still unknown, exists in the blood of cases and in the serum, even after filtering, during the first three days of the disease.

These facts have been repeatedly corroborated and, based upon them, all the successful campaigns against yellow fever have been carried out, extirpating the disease from Cuba, Mexico, Ecuador and the United States. It was also apparently eradicated from Brazil, but during the last two years very evident epidemics have cropped up there, causing justified alarm to other countries. The construction of the Panama Canal under the sanitary control of Colonel Gorgas was possible only because he applied the measures derived from the Army Board's findings.

Within the last three years it has been discovered, by working parties from the Rockefeller Foundation, that certain Asiatic monkeys (*M. rhesus*) are susceptible to yellow fever, so that now it is no longer necessary to resort to human subjects for all kinds of experiments that are being tried out in an effort to elucidate the causative germ and certain qualities of the virus (poison) of the disease. These experiments have brought out many interesting facts with relation to the origin and the spread of yellow fever; for instance, it has been possible to infect the special kind of monkeys by just rubbing the virus upon their skin; also by the bites of mosquitoes of other varieties of the same family, not previously tested for lack of subjects; also that the virus may be kept for many days, under certain conditions, without losing its infective power, etc. But nothing so far obtained can be said to have been of any practical value, that is, of value in preventing or curing the disease, and all measures to-day, after 30 years, must be based upon the discoveries of the Army Board in 1900–01.

It is possible, however, that in the near future the injection of convalescent monkey serum may be found useful in offsetting an attack of the disease, and all those in contact with it or working in the laboratory should be protected in that manner. In the treatment of the disease, large massive doses of the same serum are likely to prove beneficial, but there is much clinical and experimental work to be done before these points are made sufficiently clear.

To-day we must rest content in the fact that in civilized communities yellow fever should not be held in fear; its elimination is a matter of few days after its discovery, if attention is directed intelligently and persistently towards the destruction of mosquitoes (by fumigation) and their breeding places (by suppressing all water containers liable to receive their eggs, by proper drainage and continued vigilance), since the patients themselves are of secondary consideration.

In Panama, Cuba and the United States the menace of yellow fever is no longer cause for worry or alarm since the sanitary authorities are ever on the alert and have demonstrated that they can successfully cope with it. In Brazil the work of sanitation is much hampered by the racial conditions of the people, the difficulty in educating the population so as to cooperate with the health authorities. The same handicap is found in all wild or semi-civilized states, and they are the ones that are keeping up the infection in the form of endemic, continuous, yellow fever cases. At present the fever can be found only in West Africa and Brazil, the disease having disappeared from all its formerly well-known centers or foci.

FINALITY IN MATHEMATICS

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THE number of the developments which seem to have reached their final stages appears to be relatively larger in mathematics than in the other broad scientific subjects. For example, the formulation and the proof of the Pythagorean theorem as found in Euclid's "Elements" are still satisfying and inspiring. The various extensions of this theorem and the numerous proofs thereof are evidences of its important connections with other mathematical developments, and in this direction there is no indication of finality, but those who are satisfied with the theorem itself and a single proof thereof may see in Euclid's presentation a final accomplishment. This presentation seems to represent a culmination of efforts extending through more than a thousand years as may be inferred from the wide-spread appearances of the so-called Pythagorean triads and the recently discovered uses of the theorem even by the ancient Babylonians.[1]

Text-books have contributed powerfully towards finality of results in elementary mathematics since they tend to establish a kind of intellectual currency with respect to results which are commonly regarded as fundamental. There are various obvious similarities between intellectual currencies and financial currencies. In both cases standard forms are commonly accepted at their face values without considering the real underlying foundations. One wide difference between these currencies is that while every one has the right to try to originate intellectual currencies the various governments usually reserve for themselves the right to originate their own financial currencies. The establishment of the intellectual currencies is one of the most important and also one of the most complex activities of human society, resulting from diverse and multitudinous contributions of very unequal values. In fact, changes here are sometimes so gradual as to become tangible only when periods separated by long intervals are considered.

The question of finality is especially interesting in connection with the great scientific production of our times. If it could be assumed that the majority of the new results which are being announced are in their final forms, the magnitude of the task of future investigators to become familiar with the earlier attainments would appear to be almost appalling, especially in a subject like mathematics where connectivity has always been emphasized. On the other hand, if it can be assumed that the bulk of this production represents an intermediate stage of developments which can later be viewed from a simpler and much more general point of view one can look forward to the future investigators with a much greater complacency of mind. Various early developments in mathematics tend to inspire this complacency notwithstanding the fact that brief historical statements relating thereto are often misleading.

In particular, it is often stated that R. Descartes invented analytic geometry, but developments along this line were neither inaugurated nor extended to their final stages by R. Descartes. One of the profoundest facts of the history of mathematics is that geometry and algebra seem to have influenced the

[1] *Cf.* O. Neugebauer, *Quellen und Studien*, 1: 81, 1929.

development of each other since the beginning of mathematical records. In particular, it is commonly agreed that in the second book of Euclid's "Elements" we find a kind of geometric algebra, while some geometric problems were solved algebraically by F. Vieta, and he has often been regarded as the founder of algebraic geometry. From this point of view developments in geometric algebra as well as in algebraic geometry preceded the times of R. Descartes. On the other hand, the development of most of the standard forms of our modern analytic geometry, such as the usual forms of the equations of the tangent lines of the conic sections, appeared long after the times of R. Descartes.

Even as regards rectilinear coordinates, which are commonly called Cartesian in honor of R. Descartes, he was far from the modern general views. In particular, he never used negative abscissas. If our modern students of analytic geometry were not allowed the use of negative abscissas they would find themselves greatly hampered, and if R. Descartes could take one of our modern final examinations closing a first course in analytic geometry, with all the knowledge he possessed relating thereto while living, it is likely that most of our instructors would be inclined to flunk him. This statement is not intended to detract from his great mathematical abilities and deep insight, but it aims to convey a correct impression of some of the limitations of his actual knowledge along the lines whose development he influenced profoundly.

The work of R. Descartes in analytic geometry is conspicuous for its lack of finality. Such a fundamental statement as the one that every equation of the first degree in two variables represents a straight line and that every straight line may be represented by such an equation does not appear explicitly therein. On the contrary, such apparently final statements are usually especially emphasized in our modern works on this subject. At the close of his "La Géométrie" R. Descartes stated that he hoped that posterity would be grateful to him not only for the things which he explained but also for those which he omitted voluntarily in order to give others the pleasure of discovering them. Our modern tendency, on the contrary, is to deal with finalities in treating a mathematical question. While the congruence of two triangles having two sides and an angle opposite one of these sides respectively equal does not seem to have been considered before the beginning of the eighteenth century we now lay considerable stress thereon in our works on elementary geometry for the sake of greater completeness.

Even in such a comparatively new subject as the theory of groups there are many instances of finality. For example, A. Cayley proved as early as 1854 that there are two and only two abstract groups of order 6 as well as two and only two such groups of order 4. These are obviously final results although the methods of proof may be altered. As regards the former of these results it may be of interest to note here that twenty-four years after publishing it A. Cayley published an article in the first volume of the *American Journal of Mathematics* in which he stated that there are three groups of order 6. The fact that this error seems to have been overlooked by the editors of this periodical, as well as by A. Cayley himself when his "Collected Works" began to be published under his personal supervision at a much later period, is a very striking instance of the fact that the most obvious truths are frequently overlooked by those interested mainly in far-reaching generalities.

While many problems in mathematics are being finally solved there are others of long standing which still await solu-

tion. In fact, when a problem is finally solved it frequently points to a related one which can not be solved at the present time. As an instance of this kind it may be noted that while it is easy to determine all the groups of orders 4 and 6 noted in the preceding paragraph it is not yet possible to determine all the groups of the general order g. It has recently been shown that the number of the distinct groups whose orders are less than 100 exceeds 1,000, and the complexity of this determination increases so rapidly with the number of factors in the order that there seems little hope at present that this problem will be completely solved in the near future. Judging from the past, it appears that the number of the problems which await solution will increase with the number of those which are finally solved so that the problem of the complete mastery of mathematics is becoming continually a more hopeless one notwithstanding the encouraging increase in the number of final solutions and their growing generality.

In view of the fact that the text-books on elementary mathematics are largely concerned with material which seems to have reached its final form as regards simplicity of statement it might at first appear likely that the final text-books on certain subjects should have been reached. On the contrary, the activity in text-book production seems to be on the increase, and while some new text-books are regarded by the public as no better and no worse than their predecessors, there is frequently a decided claim therein as regards improvement. While these claims may sometimes be due to misconceptions on the part of their authors it is likely that the present text-books on elementary mathematics meet actual conditions better than those which were prepared half a century ago. At least, the final text-book on an elementary mathematical subject seems to be unlikely to appear under the present conditions relating to the adoption of such works for class use leading often to very unwise changes.

While the mathematician seems to have no good reasons for assuming finality as regards the developments in the entire field of his subject he is constantly encouraged by what appears to be permanent progress along special lines. In some fields the feasible advances are governed somewhat by practical applications. For instance, the extent of the tables of logarithms has been largely determined by the degree of accuracy attained in measurements. In more recent times the determination of all the abstract groups of given low orders has been affected by the supposed usefulness of these groups in the general theories as well as in hope that use could be made of them in certain fields of applied mathematics. Mathematical investigators are, however, more commonly governed by the desire to secure greater harmony in the developments known to them than by the hope that their results may become useful. Here our esthetic nature furnishes the main motives and practical applications have but little influence. In this direction finality seems to be as much out of the question as finality in music.

In recent years a large amount of source material relating to the history of mathematics has been collected, and such material is frequently final as regards the particular sources in question. On the other hand, historical statements based thereon are apt to be greatly affected by new discoveries relating to the subject. For instance, the opening sentence under the heading "Mathematical Societies and Periodicals" in the fourteenth edition of the Encyclopaedia Britannica is as follows: "The number of mathematical societies, clubs and circles organized since the early one at Hamburg in 1690 is exceedingly large,

but the number of mathematical periodicals since the seventeenth century is very much larger." It is obviously much easier to secure information in regard to the mathematical periodicals which were soon discontinued than in regard to the mathematical clubs which were soon abandoned. Hence it would appear possible that the quotation under consideration might be affected by later discoveries and more complete enumerations along these lines.

Those who are not familiar with the recent activities in the history of mathematics might be inclined to think that the history of ancient mathematics had reached a stage of finality, but it is just along these lines that some of the most striking and revolutionary announcements have recently been made. The recent publication of a translation of the "Rhind Mathematical Papyrus" by A. B. Chace, of Brown University, is evidence of renewed interest along this line in our own land, and the periodical to which we referred at the close of the first paragraph is largely devoted to recent developments relating to very ancient mathematics. For instance, it is stated on page 80 of this periodical that the ancient Babylonians possessed a complete formula for the general solution of the quadratic equation. Their restricted knowledge of numbers made it impossible for them to use this formula in all cases. Serious misstatements relating to the attainments of ancient mathematicians have often resulted from a failure to discriminate between the special use of a rule which is equivalent to a general modern formula and the extension of the number concept so as to make this use always possible. In the case of the quadratic equation thousands of years seem to have elapsed between these two stages.

Finality is probably one of the most powerful incentives for the mathematical investigator since it dignifies the object of his investigation enormously when he feels that his results will be final as well as enduring. As a matter of fact, however, the element of finality is apt to be only transitory since new possibilities for advancement are likely to appear even before the goal in view is reached. On the other hand, while vistas of further desirable advances may present themselves there usually is a sense of enduring progress attained and of special elements of finality in the form of theories or methods which are sufficient to incite to a repetition of effort. Thus mathematical progress seems to have been made with great variations in speed and in the utilization of earlier attainment for thousands of years but never before with such an amazingly growing number of participants as during the last century. This great augmentation of efforts tends to exhibit increasing resources rather than finality as regards possible advances.

DIFFUSION, OSMOSIS AND OSMOTIC PRESSURE

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WHAT is the reason that an odor spreads so rapidly through even the still air of a room? How is it that the oxygen of the air gets into the blood, and the carbon dioxide gets out, even though there are continuous membranes between? Why are wilting flowers, celery or salad plants revived by being placed in water? Why or *how* does sap rise in a plant?

Then there is the question as to the particular manner in which the digested food gets into the blood stream, and from the blood vessels to the tissues where it is needed. At other points of the body certain portions of the blood are being separated out as secretions. How? It is not simply a question of soaking through a leaky wall, as water soaks through a towel. At any particular point certain substances only are passed through, others are held back to be passed through apparently the same kind of a partition somewhere else.

When a piece of fresh meat is placed in salt, the fibers soon become shriveled and hard from the loss of the juices, which go to liquefy the salt. But the water may pass *in* through the same kind of a membrane with equal ease in other circumstances. Oysters are "fattened" by being placed in a vat of fresh water.

Fresh fruits in a thick sugar syrup and dried fruits in fresh water show exactly the same behavior, losing water in the first case, gaining water in the second. It seems to be merely a case of the passage of water from a region where it is plentiful to where it is not. But what is the mechanism of the exchange through the partition?

If I pour an ounce of bird-shot into a box, they roll down into the lowest corner and stay there. But if I put an ounce of gas into an empty room, immediately it expands to "fill" the entire space. How is it done? As suggested in the opening sentence, the action is similar when other gases are present in the room. The action is merely slowed up, but the mixing proceeds. How does it come that the gases of the atmosphere are uniformly mixed together, and not stratified according to density, as a mixture of bird-shot and sawdust would be if shaken up together?

Last week I put a few crystals of copper sulphate in the bottom of this tall jar, and covered them with water. The blue color of the water in the bottom of the jar indicates that solution has already begun, and the heavy copper sulphate has risen against the force of gravity. Next week it will be higher, next month higher still, and so on until there is as much of the blue color at the top as at the bottom of the jar. It may take a long time, but the tendency of the copper sulphate to go from where it is to where it isn't is just as evident as in the case of the lighter liquids and gases already mentioned. How does it do it? For that matter, how does any solid dissolve?

We shall get a vivid picture that connects all these apparently unrelated phenomena if we accept the kinetic molecular phenomena of matter, according to which all matter is made up of small particles (molecules), each one going as fast and as far as it can, *i.e.*, until it strikes something. The mean velocity of the molecule (or rather the mean square of the velocity) is a measure of the temperature; all the above

phenomena are accelerated by a rise in the temperature. The total kinetic energy of the moving particles ($\frac{1}{2}$ Nmv^2, where N is the total number, m the mass of each molecule and v^2 the mean square of the velocity) constitutes the heat energy of the body. It follows that if two gases are at the same temperature (have the same kinetic energy) the molecules of the lighter gas have the greater velocity. In fact, the molecular weights are found to be inversely proportional to the squares of the rates at which they diffuse through a porous partition. *E.g.*, oxygen molecules are sixteen times as heavy as those of hydrogen, hence the latter should work their way through small openings four times as fast as the former. Here is a little inverted cylinder of unglazed porcelain, closed by a rubber cap. From the cap a glass tube twelve or thirteen inches long extends downward into a vessel of water. Now I will bring down over this cylinder an inverted beaker and pass up into this a stream of illuminating gas consisting mostly of hydrogen and another light gas, carbon monoxide. Almost immediately, bubbles appear at the opening of the tube, showing that more gas must be passing into the cylinder than is passing out. Now I remove the beaker with its atmosphere of lighter gas, and the bubbling stops. Soon the water begins to rise in the tube, showing a diminution of pressure inside the cylinder to less than atmospheric. The movement in both cases depends on the relative amounts of hydrogen and air on the two sides of the partition. When there was more hydrogen outside, the balance of exchange was *in;* when the hydrogen inside had accumulated and there was none outside, because the beaker was removed, the outward flow predominated. It mattered not that at the beginning the total pressure inside was equal to that outside; to the hydrogen molecules, the space inside was empty—the *partial* pressure of the hydrogen there was zero—and the presence of other molecules, like the presence of the aborigines before our Pilgrim fathers, was only a temporary hindrance. Immigration of the hydrogen molecules would have gone on until hydrogen concentration on one side of the partition was equal to that on the other.

The kinetic molecule theory offers so perfect an explanation (*i.e.*, mechanical picture) of this experiment that we have no hesitation in applying it with equal confidence to all the other cases mentioned above, and with equal success. But the theory does not rest upon this sort of proof. If we imagine an elastic box containing an immense number of tiny elastic spheres, bouncing about and hitting the sides of the box and each other, and then apply the most rigid mathematical laws to the results of their behavior we shall find that (1) They will exert a uniform pressure on the sides of the box (just like a gas).

(2) If the volume of the box be increased, they will immediately occupy the additional space, but the pressure will be diminished (just as in the case of the gas).

(3) The product of the pressure and the volume will be a constant quantity (just as in the case of a gas—Boyle's law).

(4) This constant is two thirds the total molecular kinetic energy of the system, or PV = $\frac{2}{3} \times \frac{1}{2}$ Nmv^2. (The pressure of a gas multiplied by any change of volume it may undergo is the amount of work done *by* or *on* the gas. This equation has been of immense importance in thermodynamics. The fact that it has not been found wanting in spite of so many demands made upon it indicates that it must be founded on something fundamentally true.)

(5) If we make the further assumption that these tiny elastic balls have an attraction for each other, also that they must occupy some space (have volume),

we find it necessary to introduce certain modifications that convert $PV = K$ into Van der Waal's equation, an expression that describes the behavior of a real gas over a much greater range of conditions than Boyle's law can.

(6) If we imagine the velocity of the particles to be increased, the pressure will be increased in proportion to the square of the velocity. (Charles' law for gases: "Pressure varies as absolute temperature." Remember that the velocity squared is proportional to the temperature.)

In spite of this strong evidence of the probable correctness of our modern view of matter, there remains the skeptic's final retort, "Show me a molecule and I will believe." We may as well admit that this is quite out of the question for the vast majority of the molecules, for they are all smaller than half the wavelength of light (say from perhaps 5 μμ down to 0.2 μμ, while light-waves run from 390 μμ to 700 μμ. A millimicron is about 4 ten-millionths of an inch). But particles much smaller than the smallest visible under even the best microscope made, far too small to *reflect* waves of light that can form an image, may, if illuminated transversely, and with a dark background, *diffract* the light and appear as bright points. Dust motes floating in a beam of light shining through an otherwise dark room or a searchlight shining through a fog illustrate exactly what I mean. Complicated methods have been devised for measuring the diameters of these particles, and it has been found that the smallest visible in this way run down pretty close to some molecular dimensions, though we may as well admit that they are thousands of times larger than such molecules as the simpler gases.

But now glance through this low-powered microscope (10x) if you wish to see what I am talking about. Under the lens is a little chamber into which I have drawn a whiff of smoke. It is illuminated by a tiny lamp off at one side, the light of which is condensed into a cone (Tyndall's cone) directly at the focus of vision. Without the microscope you see merely the phenomenon described above. But through the microscope you see a multitude of little stars drifting about, appearing and disappearing as they rise above or fall below the focal plane. But also you notice something else. Every one of these points is aquiver with a motion too rapid for the eyes to follow; all we can distinguish is the fact that there is continual motion. From the name of the botanist Brown who first saw these motions they are called Brownian movements. He thought for a long time that the motions indicated life in the particles, but we now have a better explanation.

Go back to the kinetic molecular theory; recall the dance of the molecules, and the thumping every surface is continually receiving from them. For the smallest visible particle, there will be thousands of blows per second, coming from all directions, but *on the average*, as many from one direction as from another, so that they cancel out, and the body feels a uniform pressure on all sides. But when we get down to particles as small as these we are looking at, the blows on one side in any instant may very likely exceed those on the other side by enough to give the particle a distinct shove. It is this continual knocking about due to the rain of blows from the molecules that you are now observing. In other words, the particle of smoke is so near the size of the molecular Romans surrounding it that it does as the Romans do. In fact, we may call it a huge molecule, and we have here ocular demonstration of the kinetic molecular theory.

I can not tell you here how rigidly this phenomenon has been subjected to mathematical analysis, or how com-

pletely it has stood up under tests; simply that we are, by this time, as thoroughly convinced of the existence and the behavior of molecules as we are of the swarming aggregates of molecules that jostle us on State Street at five o'clock. The rapidity of expansion of gas into an empty space is an indication of the rapidity of movement of the molecules. As may easily be shown from the gas equation, $PV = \frac{1}{3} Nmv^2$, the lightest molecule, that of hydrogen, has an average speed of a mile a second or more at the freezing-point of water. That diffusion takes place more slowly when matter is already present is to be expected, but that it takes place at all under these conditions is proof that not all the space is occupied by the molecules. Comparatively speaking, there is more space, on the average, between the molecules of air than there is between the earth and the moon, for while the distance to the moon is only thirty times the earth's diameter, the average distance between the molecules of air is about one hundred diameters of the molecules. In the case of liquids, not more than 75 per cent. is empty space, so diffusion is still slower. The molecules of solids are not much closer together than the molecules of liquids, but there are forces acting here that we scarcely find a trace of in the case of liquids, which slow up molecular motion still more. But that it is still present is shown by the fact that a bar of lead, resting on a plate of gold for a year or so, was found to have more gold in it than was found when the experiment began.

Let us return to our solution of copper sulphate. We see the progress of the copper sulphate into the pure water, but we do not see that the pure water is also passing into the region occupied by the solution. Water molecules are passing in all directions, of course, but the presence of the copper sulphate has "diluted" the water—reduced its concentration in other words—so that there is a *stronger* water current towards the solution than away from it, and this will continue to be the case until the copper sulphate and the water are thoroughly mixed all over the space occupied by them. If we want to make this current of pure water toward the space occupied by the solution evident, it will be necessary to hold back the molecules of the solute. This I have done in this osmosis cell, ready made for me by mother nature. I have scooped out the inside of this carrot, filled the hollow with a strong sugar solution and stopped it up with a cork. A glass tube passes through the cork into the solution. The carrot is then placed in a dish of pure water. In a very little while, the rise of the liquid in the tube indicates that pure water is passing *in* more rapidly than the sugar molecules are able to pass *out*. In fact, we have duplicated with liquids of different densities our experiment with the gases of different-sized molecules and the porous cup, and made visible what is taking place in every living plant all summer long. The root hairs are bathed in a dilute nutrient solution of mineral salts. Within the cells of the plant is a more concentrated solution—concentrated because of the continual evaporation taking place through the leaves and because of the building up of large molecules of starch and sugar out of water molecules and carbon dioxide from the air.

The white corpuscles of the blood are simple cells floating in the blood serum which is, essentially, a 0.9 per cent. salt solution carrying a host of other things in suspension. The water can pass through the walls of the corpuscle quite freely, the salt very slowly, but the protoplasm inside the cell can not pass out; in other words, we have here a cell with semi-permeable walls. As far as the salt is concerned, the cell contents and the surrounding serum are of equal concentrations; we speak of them as being

isotonic. If the cell be placed in a slightly stronger salt solution, water passes out more rapidly than it can pass in, and the cell shrinks and becomes wrinkled; if it be placed in a weaker solution, it loses salt, absorbs water, swells and bursts. Distilled water causes similar action when taken into the body. It has "taste"; it dissolves so much from the epithelial cells of the mouth that the taste nerves rebel. There is a spring in southern France known as the Poison Spring, but it contains no poison—the water is purer than the ordinary distilled water. The extraction of sugar from the sliced beet is not a mere washing out of the sugar, as dirt is washed from clothes; the sugar diffuses through the cell walls as molecules. Beets must not be crushed, as one crushes cane, for crushing the cells would release obnoxious compounds that could not later be got rid of. Fortunately, the cell walls are impermeable to them. An interesting physiological experiment may be mentioned here.[1] If the uterus of a guinea-pig be suspended in an isotonic saline solution (0.9 per cent.) certain drugs will cause it to contract. If the salinity of the surrounding liquid be increased, the effect of the drug is diminished, stopping entirely at a concentration of 1.3 per cent.; but, in a weaker solution, the action of the drug is increased. In the stronger solution, the currents are from the cell *out;* in the weaker solution, the currents are *in,* hindering the absorption of the drug in one case, helping it in the other. A dose of bicarbonate to remedy the acidity of the stomach is taken with a little water; the action takes place outside the cell. But if a drug is to be absorbed by the cells, take much water, so as to decrease the tonicity of the fluids in the digestive tract and set up a current *into* the cell, which will carry the drug along with it. Some drugs, such as magnesium sulphate, are not absorbed by the cell walls; we say the cell walls are impermeable to them. To dilute the contents of the intestines, an abundant secretion is poured forth from the walls. This explains the laxative effects of Epsom salts.[2] The loss of water by a cell increases the concentration of its contents, and mass action may accelerate chemical processes within the cell that would take place very slowly or not at all under ordinary conditions.[3] This passage of molecules through a membrane impermeable to other molecules that may be present is called *osmosis,* a term we shall find convenient.

Suppose that our tube, in the carrot experiment, had been bent at right angles. We should then have had a very fair imitation of an excretory gland in action. Of course, the action would have stopped as soon as the sugar had been washed out of the cavity and the liquid inside had become of the same concentration as that outside. Or it might be that the cell could build up, out of the small molecules that could enter from the outside, larger molecules to which the cell membrane was not permeable. ("Larger" or at any rate *different.* It is not always a matter of size that determines whether or not a molecule shall pass through a certain membrane.) Now if the other end of the cell is open, or closed by a membrane permeable to these new molecules, we shall have a steady current through the cell, and a model of a secretory gland, such as the salivary or pancreatic.[4]

A semi-permeable membrane is not absolutely necessary for the demonstration of osmosis. If alternate drops of solutions of higher and lower tonicity be placed in a capillary tube, and the lengths carefully measured, it will be found that the more concentrated will become longer at the expense of the less concentrated. The surface of the liquids

[1] Bayliss, "Physiology," p. 163.

[2] Findlay, p. 53.

[3] Bayliss, p. 63.

[4] Bayliss, p. 334.

serves as the semi-permeable membrane. Two beakers, one filled with pure water, the other with a salt solution, if placed under a bell jar, will exhibit the same phenomenon, water distilling over from the pure water to the solution.

To return to our carrot experiment. As the column of liquid rises higher and higher in the tube, it is evident that there must be a strong pressure exerted on the inside of the cavity, or "cell." This has been named osmotic pressure, since it seems to be caused by osmosis (passing through) of the water molecules into the cell faster than they can pass out. But this, in turn, is due to the fact that the sugar molecules diluted the water (reduced the concentration) so that not as many molecules of water per second struck against the inside of the cell as against the outside—per square centimeter, of course. When equilibrium has been attained, if the cell is strong enough to stand such a pressure, as many molecules will strike from one side as the other, so the net water pressure will be zero, and the extra internal pressure will be due to the sugar molecules bombarding the cell walls, trying to get out but unable to. Haldane[5] defines osmotic pressure as the pressure of the water outside the cell, but in this he is in disagreement with the majority of physicists. It is a mere matter of nomenclature anyway; there is no disagreement as to the amount or the meaning of the pressure itself.

It is thus seen that osmotic pressure is due to the same thing that brings about solution pressure of a dissolving solid or vapor tension of an evaporating liquid or the gaseous pressure of a permanent gas. And that is the motion of the molecules. The pressure is not caused by the membrane any more than atmospheric pressure is caused by the barometer, but to make manifest and to measure the pressure we have to keep the solute from streaming into an unoccupied region, just as we measure atmospheric pressure only by preventing the air from streaming into a vacuum.

When we come to measure this osmotic pressure we run across some surprising relations. Measurement is made by allowing the water to rise as far as it will in a vertical tube attached to a cell containing the solution, and measuring the hydrostatic pressure, or by increasing the pressure within the cell by means of a manometer until the inflow of water from the outside is prevented. This is the better way, since the concentration of the solution is not changed.

Let us make a solution of sugar, 15.3 gms per liter (about a heaping teaspoonful to a quart of water). It will exert a pressure equal to atmospheric. So also would 0.0893 gms of hydrogen if confined in an equal space. But 15.3 is 171 times this weight of hydrogen and the molecular weight of sugar is also 171 times the molecular weight of hydrogen so there must be equal numbers of sugar molecules and hydrogen molecules. In other words, we have equimolecular concentrations producing identical pressures. Does it not suggest that they are in the same condition, the gaseous?

If we change the volume of the solution in which we have our 15.3 gms of sugar, we change the pressure in the inverse proportion. But this is nothing but Boyle's law which, we have seen, describes one of the most characteristic properties of gases. Or if we change the amount of sugar dissolved in one liter of water, the pressure changes in the same proportion. If we dissolve equimolecular quantities of other substances in one liter of water we get exactly the same pressure, always providing, of course, that (as often *is* the case) there is no reaction between the water and the solute that changes the number of particles in solution. Here we have Avogadro's law for gases—that equal numbers of molecules in the same vol-

[5] Rogers' "Physiology," p. 42.

ume exert the same pressure at the same temperature. If, keeping the concentration the same, we measure the osmotic pressure of a solution at zero and again at one degree above zero we find that it has been increased by one two-hundred-seventy-third. Here we have Charles' law for gases.[6]

We have mentioned the modification of Boyle's law (Van der Waal's) made necessary by the attraction of the molecules for each other, and the fact that the molecules themselves must occupy some of the space in which they are moving about.

In the case of solutions, slight deviations from the laws mentioned in the last paragraph can be shown to fall into an orderly scheme if we make due allowance for these factors and a few others that would naturally be of much more importance in solutions than in gases. (Thus a sugar solution seems to act as if each molecule of sugar were associated with five water molecules.) We can not entirely neglect the influence of the membrane or the electric charge it may carry. This will be discussed later. Membranes are colloids, and little is known yet as to what might be expected of their behavior. But when we ascribe numerical values to such of these factors as is possible we get an equation of the same form as Van der Waal's.[7]

Fortunately we are not obliged to depend upon an actual measurement of osmotic pressure in comparing the tonicity of different solutions. It is a difficult operation. But theoretical considerations showing that the boiling-point, which is only the temperature at which the vapor pressure becomes equal to the atmospheric, is dependent upon the osmotic pressure are borne out by experiment. We have already pointed out that the surface of a liquid may be looked upon as a semi-permeable membrane. Evaporation is merely the passing through this membrane of those molecules that have sufficient kinetic energy to overcome the backward pull of the other molecules in the body of the liquid. If there are molecules of another substance present, too large or too heavy to break away, they are taking the place of just that number of molecules of the solvent which might be passing out through the surface. In order to make up for this, the temperature must be raised, increasing the average kinetic energy of all the molecules, until the same number of molecules are able to leave as before. As a matter of fact, one gram molecule of any non-volatile substance will raise the boiling-point of water 0.52°. This rise of the boiling-point varies with the solvent, being generally more for other solvents than it is for water.[8]

Similar considerations apply to the separation of the pure solvent from a solution by freezing. The surface of the crystal acts like a semi-permeable membrane. In this case, only those molecules with less than a certain minimum of kinetic energy adhere to the surface of the crystal and give up their heat of solidification to the main body of the liquid. When another sort of molecule is present which can not crystallize, the temperature must fall (average kinetic energy must decrease) until the same proportion of the total number of molecules is below the freezing temperature as in the case of the freezing of the pure solvent. The depression of the freezing-point for a gram-molecular solution in water is 1.84° Centigrade. Determinations of the freezing-point and the boiling-point of various solutions are constantly being made by the chemist in the measurement of molecular weight, or, in other words, in learning the comparative numbers of particles that function as molecules in a solution. Such determinations are easy and accurate.

[6] Haldane, *Biochem. J.*, 12: 464, 1918.

[7] Bayliss, pp. 150, 152, and Rogers' "Text-Book of Comparative Physiology," p. 48.

[8] Morgan, "Elements of Physical Chemistry," p. 191.

The electrical condition of the membrane as affecting osmosis has been referred to. It must be due to this that many membranes have been found to transmit ions of one kind but not of another. For instance, the stomach walls seem to be permeable to the positive ions Na and H, but not to the negative ions Cl and HCO_3. Only if the food contains NaCl is HCl to be found in the stomach,[9] its presence being explained as follows: The NaCl dissociates into positive Na ions and negative Cl ions, the former diffusing through the walls of the stomach into the blood stream only as fast as they are replaced by the positive H ions from the blood. The latter arise from the dissociation of the carbonic acid in the blood, and this comes from the CO_2 thrown into the blood as the result of the general oxidation processes of the body. Actually the acid never accumulates in the blood, for this reason among others that the sodium ions unite with it to give $NaHCo_3$, a salt with an alkaline reaction.

Not always is osmosis through animal membrane so easily explained as this, however. The walls of the blood cells are permeable to both H and Cl ions, impermeable to Na and HCO_3 ions. One might be inclined to say it was a question of weight, but the skin of the frog allows Na ions to pass out, but not in, while K and Cl ions may pass either way. Probably no single explanation is sufficient.

Fertilization of the ovum by the sperm-cell is accompanied by changes in the permeability of the wall, and part of Loeb's success in artificial parthenogenesis was due to his discovery of the salt solutions that had the correct osmotic pressure to stimulate natural conditions. Not only in the life history of the cell but even in its very beginning we find osmosis playing a very necessary part.

One thing more remains to be said. Recalling that PV has the dimensions of work, it is evident that any change in the osmotic pressure within a cell implies that work is being done, either by or on the system. This can be calculated from the well-known expression for the work done by a gas when its volume is changing isothermally from V_1 to V_2 $(= dV)$, $W = PdV = RT \log_e (V_1/V)$, T being the absolute temperature and R a constant depending on the units used. For instance, the kidneys separate from the blood a liquid of much higher osmotic pressure than the blood itself. Incidentally, we may note that the kidneys seem to be the regulators of the osmotic pressure of the blood. Whereas it is usually about 0.3 molar, extirpation of both kidneys may run it up to five or six times that amount. But the osmotic pressure of the urea and salt in the kidneys is about molar. That is, the concentration has been about tripled. From the above equation, it is easy to calculate the amount of work that the kidneys must do on the contents of the glomerules in order to keep up the excretion of the urine. Where this energy comes from and how it is applied to the job in hand is pretty hard to understand. There can be no disputing the fact that it is being done. To shut our eyes to the fact is equivalent to being uninterested when we see water flowing uphill.

We may close by calling attention to the fact that changes of concentration (changes of osmotic pressure) within the cell may take place by the splitting of large molecules into smaller ones, or the reverse. In the latter case, the number of molecules being less, the osmotic pressure is lessened, and the result is that energy is set free to be used in the various processes of the body needing energy. Doubtless a more thorough study of the osmotic pressure of various cells of the body will go far toward solving the mystery of how the body utilizes the chemical energy of the food partaken of.

[9] Findlay, p. 51.

PHYSICAL TERMINOLOGY

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I

THE saying that "one ought not to investigate things from words, but words from things, for things are not made for the sake of words, but words for things" is credited by Diogenes Laertius to Myron. The fertility of this view is exemplified in the success of modern science. Words must be regarded as a means and not as an end. But this does not signify that, in the study of things, words should receive no attention. Myron implied that they should be investigated. In the natural sciences, words are not, of course, fundamental in the sense that instruments, processes and laws are fundamental. Yet, aside from mathematical symbols, they are the only means we have for giving unique designations to the things that are more important. These unique designations constitute the terminology of science.

Terminology must exist but it apparently has little to do with the nature of things. Consequently its problems usually fail to attract the scientist. The specialist can afford to be indifferent to words simply because to be a specialist is, in a sense, to have transcended such matters. Faulty terminology may become very troublesome in fields like spectroscopy and organic chemistry, or in attempts to interpret the material of one's own field in terms of the material of another. But in most of the older parts of physical science, the trained man experiences little difficulty. He knows that *radiation* denotes one form of energy, and *heat* another, so that *heat radiation* can not very well denote both. Without discomfort he employs such meaningless or ambiguous terms as *dark heat, gram molecule, vapor tension, latent heat, chemical ray, electric capacity, positive ray, electric force, electromotive force* and *permanent gas.* He realizes that many of these terms should be regarded as obsolete, even though the text-book writers do continue to use them. He knows that most of them have histories that clearly account for their existence. No matter how faulty a term may be, it is to him simply a symbol for a very definite concept and is without the flexibility of connotation that is characteristic of words in general.

This is all very well for the specialist. Yet if physical terminology is unnecessarily faulty and complicated, and there are reasons to believe that it is, then the scientist should give some attention to the mattter, if only to the end of its quicker disposal. One way to avoid boresome dialectic discussions is to have nothing to discuss.

Then, too, the general reader and the student not specializing in science need some consideration. Although they may not be important factors in scientific progress, nevertheless it is generally held that some knowledge of natural science is good for them and in some instances this knowledge is required. Hence some one is under obligation to render the elements of science intelligible. Not superficial but intelligible; to confuse the two is to confound vagueness of expression with profundity of thought.

II

The contention that a revision of existing terminology would materially simplify physics and chemistry for the beginner is borne out by the various vocabulary studies made by the educationalists. These studies seem to lead to the conclusions that the technical vocabularies of the elementary text-books are too

large and that there are too many new terms that are inadequately discussed. What naturally is not revealed by such studies is that there exist many terms that are inherently faulty or that need discussion for purely scientific reasons. This latter contention gains weight when one attempts to analyze terms in the light of their possible faults.

The remainder of this section is devoted to a brief discussion of some of the results of such an analysis. To facilitate discussion the various faulty and troublesome terms are divided into three classes: terms that should be discarded; terms requiring careful definition or special comment, and terms having more than one meaning.

There are many terms that should be discarded as working terms because they have synonyms that are less ambiguous or more meaningful. Among these are *sensible heat, theoretical mechanical advantage, uniform motion, mechanical equivalent of heat* and *latent heat.* Still other terms, such as *positive electron, quantity of magnetism* and *therm,* are not in themselves objectionable but should be discarded because there already exist more widely used terms having identical meanings. When one of these terms, such as *latent heat,* has unusual historical interest, there can not be any objection to its retention in the text-books, but it should be regarded as obsolete and labeled as such.

Many terms in common use represent concepts that are not important enough in the physical sciences to deserve special names. Some of these terms have been borrowed from the industrial arts, where indeed they may be useful. Some found their way into the text-books at a time, now remote, when apparently there was a want of subject-matter suitable for the elementary courses. Others originated in the anomalies and paradoxes of science, which, as Faraday has said, "only remain anomalies to us while we continue in error." Still other terms that may be regarded as superfluous, at least so far as the general student is concerned, are those which represent the reciprocals of quantities already having acceptable names. Examples of these various types of unimportant terms are: *anion, centrifugal force, diathermanous, conductance, magnetic length, consequent pole, penumbra, simple vibration, specific volume, concurrent forces, water equivalent of a calorimeter* and *anomalous dispersion.*

Needless contractions, such as *gravity, photo-current* and *radio-element,* form still another group of superfluous terms. Such contractions are often used interchangeably with their parent terms but with only the latter defined. Yet to the beginner, a contraction is a new term, for he has been taught that two scientific terms which differ only slightly in form, for example *resistance* and *resistivity,* usually represent unlike though probably related concepts.

To label this or that term as unimportant or superfluous is, of course, to invite an argument. The specialist is likely to regard most of the terms of his field as important, and rightly, since for his purposes so many of them are useful. For the general student, however, there are literally thousands of terms which might as well never be known. To select the most important terms might be a difficult task, but this should not prevent the attempt.

Particular attention should be given to terms which are undesirable but which are apparently too firmly established to be discarded readily. There are not many of them, but the few that do exist are often very troublesome. Usually they are very old or else have a popular origin and usage. Some of them originated in misconceptions of a fundamental character: *electromotive force,* which has the dimensions of energy divided by charge, originated at a time when force and energy were badly confused; *atomic weight,* which is neither a

weight nor a fundamental atomic property, originated when weight and mass were confused and when atoms were studied collectively, and not individually. *Four-cycle engine*, referring as it does to an engine having four strokes to a cycle, is an example of a misleading term having an established commercial usage.

Strange words also need special comment. To the beginner who is unacquainted with languages other than his own, particularly Latin and Greek, many terms seem as strange and meaningless as nonsense syllables. Several devices are available for overcoming this difficulty. It is a help to know the accepted pronunciation of a word, especially in the case of a foreign proper name. Expressions like *vernier, mass action, calory* and *fluorescence* are illuminated by their histories. *Vector, ion, colloid, hysteresis, moment* and *viscosity* gain content when their etymologies are known. Many other strange words, such as *achromatic, isotropic, amorphous* and *exothermic*, give no trouble to one who has learned a comparatively few prefixes and suffixes from which hundreds of terms are constructed. To be sure, many terms have outgrown their original meanings. *Inertia*, as Faraday pointed out, is now to be used metaphorically to express, not "idleness," but "habitude." *Atom* and many other terms have also lost their traditional meanings. To discard such terms is usually impracticable because of lack of suitable synonyms. To replace them by newly coined terms would lead to endless change and confusion, for even the acceptable terms of to-day may prove to be faulty when viewed in the light of to-morrow's discoveries. Fortunately many of them have become so completely divorced from their original meanings that trouble with them is unlikely. Thus, while the etymology of a term may throw much light on the history of a scientific concept, it is not a safe guide to modern usage. Resort to etymology simply helps to give a term content and renders it easier to recall.

Special attention should also be given in the text-books to pairs of terms like *dielectric constant* and *dielectric strength, gravity* and *gravitation*, and *thermoelectric* and *thermionic* because of similarities in form and meaning. Other terms in need of careful discrimination and comparison are *energy* and *force, energy* and *power, speed* and *velocity, density* and *specific gravity*, and *inertia* and *momentum*. One naturally would expect students to have difficulty in distinguishing these concepts, because many of them were badly confused in the early history of physical science. Unfortunately text-book writers carelessly encourage this confusion. Probably every physics text-book emphasizes the distinction in the definitions for pairs of terms like *speed* and *velocity*, and *energy* and *power*, but surprisingly few of them afterwards consistently employ these terms in their correct and separate senses.

A number of terms, of which *bar, power, Avogadro's number, molar* and *intensity* are examples, have two or more distinctly different technical definitions. Confusion often results, for the sense in which an author chooses to employ one of these terms is not always immediately evident, even to an experienced reader. In every case known to the writer, all but one of the definitions could be abandoned without the necessity of coining terms to represent the eliminated ideas. Since most of these terms are purely technical, such changes can easily be made by continuing the practice of including standardized definitions in widely used compilations like the International Critical Tables.

It is often necessary, particularly in elementary instruction, to simplify a concept and thus to employ a term in a modified sense. The occasion for this may rise because of mathematical difficulties or because of the inability of the

beginner to get an intuitional grasp of the original concept. Again, the original concept may be too general in nature for a given purpose, perhaps including insignificant or rarely observed phenomena. In this way, terms like *specific heat, electromotive force, entropy, instantaneous speed, force, temperature, calory* and *mass* come to have several definitions—an established, general definition and one or more simpler definitions. It hardly need be said that the simpler definition should represent, whenever possible, an actual special case of the established definition and that its limitations should be made as clear to the student as the nature of the subject permits.

Many words have both a popular and a scientific usage and the two usages may differ. This is the case with *work, pressure, degree of elasticity* and *action.* Many other words that the student is expected to know from his general reading or that he can find in ordinary dictionaries really are technical terms needing special explanation. One can not assume that the beginner has definite ideas about *tension, normal, rate, scientific, theory, uniform, experiment, critical, modulus* and *coefficient.* A knowledge of these familiar words usually comes about incidentally, and this informal learning too often consists, not of understanding words and situations, but of encountering them often enough to get over the unpleasantness of their strangeness. The text-books can not, of course, define every term, and if they did they would defeat one of their purposes, which is to stimulate collateral reading. Since ordinary dictionaries are not always to be depended upon for accurate definitions of scientific terms, it is unfortunate that we do not have inexpensive scientific dictionaries, specially prepared by men of science for use in elementary instruction.

The sciences borrow many words from every-day language and convert them to their own special purposes. On the other hand, many terms peculiar to science are put to non-technical uses. Both practices lead to confusion but are not always avoidable. To use a technical term in a metaphorical sense may not necessarily be a sign of bad taste on the part of the lay writer but it leads gradually to the misrepresentation of the original meaning of the term. What is worse, it tends to destroy the uniqueness of scientific language. Without a unique terminology, any field of knowledge is seriously handicapped, a fact of which the social scientist, for one, is well aware. Biologists might not have to give so much time to the correction of popular impressions if treatises on organic evolution could be clothed in the unfamiliar language of, say, mathematics. But for the blessings of a technical language the professional prohibitionists might busy themselves with relativity, or with quantum mechanics, perhaps to the end of denying us our wave-lengths, as our ancestors have been denied their tails.

Popularization of terminology leads also to a premature familiarity with strange terms which may breed contempt for their meanings. The jargon of a subject like radiotelephony, Freudian psychology or aeronautics is glibly employed by untrained persons with such apparent success that it indeed must seem to them unnecessary to acquire any fundamental knowledge of these fields. A confirmed radio addict seldom makes a graceful beginner in physics.

III

If this problem of terminology turns out to be largely a pedagogical one, it does not follow that it is a problem for the educationalist alone to solve. Physical science, fortunately, has reached the stage where the educationalist with a sense of scholarship will refuse to meddle with it without the assistance of the scientist. On the other hand, if a revision of terminology is a pedagogical

necessity, such a revision must not be attempted by the scientist alone. The demands of good scholarship extend in both directions. The specialist in science education is making progress in a genuine scientific study of subject-matter and technical vocabulary. His conclusions, when based on observation and experiment interpreted in the light of psychology, can not be ignored by the physical scientist, who above all others is in a position to appreciate the validity of such a method of obtaining knowledge. Of course it is also the pure scientist who is best able to foresee the enormous difficulty of obtaining reliable and fundamental experimental results in a field like education. Perhaps if a little more concern for these difficulties were evident in the work of the educationalist, his conclusions would be taken more seriously. However, if some of his results are untrustworthy and some of his conclusions far-fetched, they are hardly more so than were those of the early physical scientists. If his work in science education sometimes betrays a woeful ignorance of the nature and meaning of natural science, it can not be much worse than that of some physicists or chemists who attempt to teach without benefit of the principles of psychology as applied to education.

In all problems of science education, the scientist and the educationalist will eventually have to cooperate. In this matter of faulty terminology the initial responsibility rests with the scientist. He must put his house in order, for he is the only one who knows where things belong. The responsibility for correcting faulty terminology was recognized by men like Maxwell and Kelvin, both of whom were purists in matters of language. The few really outstanding text-books of physical science are usually characterized by a thoughtful selection of terminology and notation. But for the physicist and the chemist alone to determine what shall be the terminologies of their fields is not enough; technical vocabularies adjusted to the needs of various types of learners must be constructed, and here the scientist and the educationalist must work together.

Every experienced teacher knows that physics and chemistry present difficulties that are not disposed of by the assertion that there is no royal road to science. To attribute a part of these difficulties to vagueness of language is at first surprising, but it must be remembered that the terminology of established science was conceived during exploration, and the inevitable concomitant of exploring new fields of knowledge is a vagueness of ideas, with an accompanying vagueness of language. The hazy ideas of yesterday are reflected and, what is worse, preserved in the terminology of to-day. This does not provide an argument for a complete revision of terminology, but rather shows a complete revision to be impossible.

Any revision that is made will depend for its success upon the text-book authors and, still more, upon the teachers. The authors select the terms that appear in the elementary books, but the teachers select the books. The average teacher does not take kindly to a text-book that departs from the traditional type—a fact of which authors and publishers are well aware; hence innovations in language will be introduced into the text-books with caution. The chemists and the technologists are making progress in language reform through the medium of their committees on nomenclature. The technical journals can accomplish much, but they will always be hampered by the many readers who protest every attempt at language reform with the fervor of a fundamentalist. The most progress will be obtained, however, by convincing teachers and writers that a revision of terminology is practicable and that it really is an important step in making science more intelligible to the learner.

SCIENCE SERVICE RADIO TALKS

PRESENTED OVER THE COLUMBIA BROADCASTING SYSTEM

WEST INDIAN HURRICANES

By CHARLES L. MITCHELL

PRINCIPAL METEOROLOGIST AND DISTRICT FORECASTER OF THE U. S. WEATHER BUREAU AT WASHINGTON, D. C.

MENTION of the word "hurricane" is quite likely to create an uneasy feeling in the minds of people living on or near our Gulf or South Atlantic coasts, or on any of the islands that comprise the West Indies and the Bahama Islands. And there is little wonder that such a feeling should now exist, especially in Porto Rico, the Bahamas or on the southeastern Florida coast, where at least one of these destructive storms has been experienced within the last two years. It has been our experience that after a hurricane has affected a certain area the inhabitants of that same area will be quite "jumpy" for two or three years, at least, after such a visitation, and that any intimation of the existence of a hurricane, no matter how far distant, or even the rumor of the existence of such a storm, will bring a flood of inquiries for information to the Weather Bureau.

Before going further, it may be well to say something of the life history of hurricanes. It should be of some interest to know that the typhoons of the Far East, the cyclones of the Bay of Bengal, the hurricanes of Australia and the islands to the north and east thereof, and the West Indian hurricanes are all practically identical in manner of development, intensity and general direction of movement, from east to west. However, because of the deflective force of the earth's rotation, which is to the right in the northern hemisphere and to the left in the southern hemisphere, the winds circulate around the center of the storm clockwise, *i.e.*, from left to right, in the southern hemisphere, and counter-clockwise, *i.e.*, from right to left, in the northern hemisphere. Furthermore, these revolving storms of the northern hemisphere recurve toward the north and northeast and those of the southern hemisphere toward the south and southeast after moving a number of days toward the west.

All you listeners-in have doubtless experienced a state of mind known as the "doldrums," when there was a sort of sense of depression with variable decisions that lacked force and permanence. There is likewise an area, or belt, called the doldrums, over the Atlantic Ocean north of the equator, between South America and Africa, that moves northward and southward with the sun, and in this belt there is a slight depression of the barometer, that is, the atmospheric pressure as measured by the height of mercury in the barometer is lower than over the ocean, either to the north or to the south of this area. the winds are light and variable, as a rule, and local thunderstorms of the usual afternoon convectional type are rather frequent. During the seasons of the year that this belt of doldrums is quite near the equator nothing happens other than the conditions just described, largely because of the fact that the deflective force of the earth's rotation is necessary in the process of initiating a cyclonic storm, and that force is so near zero on and near the equator that no such storms can originate there. How-

ever, in late summer and early autumn, the eastern end of the Atlantic belt of doldrums is so far north that it is close to the Cape Verde Islands, which are a short distance west of Cape Verde at the western extremity of Africa. Being between ten and fifteen degrees away from the equator here, the deflective force of the earth's rotation is quite sufficient to initiate a cyclonic circulation, granting that other conditions are favorable, while at the other end of this belt of doldrums on the coast of British Guiana, South America, which is only six or seven degrees away from the equator, the deflective force is too small, so that such storms do not develop there. Other conditions being equal, the farther removed from the equator a region is, the more likely it is that a cyclonic storm will develop there. It is generally agreed by meteorologists that high temperature and high humidity are favorable to, if not essential in, the development and maintenance of a tropical cyclone. Both these requirements are fully met in the doldrums between the system of northeast trade winds of the northern hemisphere and the southeast trade winds of the southern hemisphere. When the belt of doldrums shifts sufficiently far to the north, which is in the period roughly from the middle of July to the middle of September, the southeast trade winds change their direction, on account of the deflective force of the earth's rotation, and become southwest winds when more than about five degrees north of the equator.

Between these steady winds blowing from opposite directions is the doldrum area of light variable winds, high temperature and high humidity—all favorable for the development of local thunderstorms and squalls. At times these conditions increase in frequency and intensity for a few days, and then by some so-called "trigger" action, which is not thoroughly understood but which requires winds from opposite directions over the ocean areas to the north and to the south of the unsettled region, a cyclonic circulation is somehow set up around a center in the belt of doldrums. After the wind circulation is once started, there is a plentiful supply of energy from the latent heat set free in the process of condensation and precipitation of moisture in connection with the heavy rainfall that always accompanies tropical cyclones, so that the cyclonic circulation is not only maintained, but greatly increased, both in lateral and vertical extent.

Over the entire tropical zone the movement of the air from a comparatively short distance above the surface up to two or three miles is steadily from east to west nearly all the time. Therefore, as soon as the cyclonic storm has developed upward into this steadily moving mass of air toward the west, the cyclone is carried along with about the same velocity and direction as the great air mass above, much the same as a whirlpool in a stream is not only whirling around a center quite rapidly, but is carried down the stream with the main body of water. In the case of the tropical cyclone it is carried along toward the West Indies in a direction usually slightly north of west and at the rate of movement of the main air mass above, which varies as a rule from eight to eighteen miles an hour and averages about twelve to fourteen. This progressive forward motion of the entire cyclonic storm, or hurricane as it may now be called, must not be confused with the rate of movement of the air that is moving in toward and around the center, or vortex, of the storm. It is this tremendous velocity which in all hurricanes proper reaches at least sixty to eighty miles an hour near the center, and in the great hurricanes such as those of September, 1926 and 1928, reached 125 or more miles per hour. In the great hurricane of 1928 that devastated Porto Rico, the wind in the interior of

the island south of San Juan reached an estimated velocity of about 200 miles per hour, which is the greatest experienced in any hurricane of record. This is the nearest approach to the velocity of the wind attained in tornadoes of which we have knowledge. It is probable that in some tornadoes, judging from the effects, the wind attains a velocity of 300 to 400 miles an hour over an exceedingly small area. Hence the tornado is the most violent of all storms, but of exceedingly small extent, and the hurricane is next in violence, but covers a vastly greater area and, therefore, continues for several hours instead of a fraction of a minute as in the case of the tornado.

Now as to the method of receiving and disseminating information relative to tropical cyclones. The Weather Bureau maintains a network of observing stations where twice-daily observations are taken and sent to Washington, not only on the mainland of the United States, but also in the chain of islands that extends from Haiti around the eastern end of the Caribbean Sea to Trinidad, which is near the mainland of South America, as well as in the Panama Canal Zone and along the Caribbean coast of Central America. In addition, reports are received just as often from Jamaica and the Bahamas and from several stations in Cuba. Before wireless communication with vessels at sea was established, the bureau was able to detect and follow the paths of tropical cyclones when they approached and remained near any of these reporting stations on land, but it was much handicapped when they passed either north or south of the main chain of islands, with their centers far out to sea.

In recent years, however, the vessel-reporting service of the Weather Bureau has expanded greatly until now scores of wireless reports are received twice daily from vessels in the Gulf of Mexico, the Caribbean Sea and the western North Atlantic Ocean. Each report gives, in code, the location of the vessel (latitude and longitude), the reading of the barometer, the temperature, the direction from which the wind is blowing, the state of the weather and the velocity of the wind. All these data are plotted on the principal chart on which the regular weather forecasts of the bureau are based.

A brief account of the work of the Weather Bureau in connection with the great hurricane of 1928 will indicate the great value of the weather reports radioed from ships, and also the value of warnings issued by the bureau.

As stated before, the tropical cyclones that originate near the Cape Verde Islands are carried along in the general drift of the atmosphere westward toward the West Indies. Therefore, when two vessels nearly 600 miles east of Bridgetown, Barbadoes, the farthest east of the island observing stations at the eastern end of the Caribbean Sea, radioed their morning weather reports on September 10, 1928, and the data contained therein were plotted on the forecasting chart, it was instantly noted by the forecaster at Washington that the usual undisturbed conditions over that region of the Atlantic Ocean, where steady northeast trade winds and nearly stationary atmospheric pressure predominate, had been replaced by increased wind strength and falling barometer. This meant to the forecaster that a tropical cyclone was approaching these vessels from the east or southeast, and special reports later in the day from one of these vessels enabled the forecaster to locate quite definitely the center of the storm. Incidentally, this is the farthest east that the bureau has ever been able to detect the approach of a tropical cyclone. As a rule, there are few or no vessels in that particular ocean area, and furthermore, such vessels as do traverse that region are not always able to communicate with San

Juan, Porto Rico, by radio, because of too much static. Immediately after receipt of the vessel report just referred to, an advisory warning was prepared, giving the location of the center and direction of movement of this storm, and this warning was telegraphed to all the Weather Bureau stations on the Gulf Coast and to the Atlantic Coast stations as far north as Boston, so that vessels clearing from our ports could take proper precautions, if necessary, such as delaying their departure or changing their course. The warning was also cabled to Cuba, Haiti, Bermuda and the Bahamas, and was wirelessed by the naval radio stations to all ships at sea. While the storm center was still a few hundred miles east of the Lesser Antilles (which is that chain of islands that extends from Porto Rico around the eastern end of the Caribbean Sea almost to the South American coast), the warning sent out from Washington stated that the storm would pass over those islands the following day to the north of Martinique, and the center did actually pass over the French possession of Guadaloupe, about seventy miles north of Martinique.

The same procedure was carried out twice daily until this, one of the most severe and extensive West Indian hurricanes of record, reached Virginia as a disturbance of only moderate intensity on the evening of September 19. In each warning sent out for several days before its arrival on the Florida coast, the severity and the dangerous character of the hurricane were stressed, and the location of its center and its approximate direction of movement for the ensuing twenty-four hours at least, as well as the places it would likely pass over or near and the probable time of arrival, were given.

The effectiveness of the warnings in clearing the shipping lanes is well shown by the fact that not a single vessel report was received from near the center of the hurricane from the time the first warning was issued until the storm approached the Florida coast, when one vessel, for reasons best known to its master, steamed down along the southeastern Florida coast with the knowledge that the hurricane was close at hand, and it encountered winds of force 12, which, on the Beaufort Scale, means winds of seventy-five or more miles per hour.

At least thirty-six to forty-eight hours ahead of its arrival on the Florida coast, the Red Cross officials were advised of the danger to southeastern Florida, and the suggestion was made that a force of their workers be concentrated at Jacksonville and that the local chapters in the threatened area be advised to be prepared for emergency work. Evidently such action was taken, inasmuch as Judge John Barton Payne, in an appeal over the radio for public support of the Red Cross, stated that because of the warnings of the Weather Bureau the preparatory steps advised were actually taken so that the Red Cross was ready to function immediately when their assistance was sorely needed.

When there is great loss of life in a hurricane it is invariably due to drowning in low-lying sections where the great force of the wind causes the water to pile up over the low, but usually dry, land. Hence in the 1928 hurricane only twenty-six deaths were reported in the West Palm Beach area, while around the southern end of Lake Okeechobee, where the water attained a height of ten to fifteen feet on the shallow rim of the lake, there were approximately 1,700 deaths. The velocity of the wind was practically the same over both areas—more than 100 miles per hour. The great need of aid rendered by the Red Cross is shown by the fact that 10,172 families applied for aid during the month following the hurricane, about two thirds of this number being in Palm Beach County, which includes the Okeechobee area.

While eighty-four tropical cyclones of

hurricane intensity have reached the coast of the United States between Cape Hatteras and the mouth of the Rio Grande during the last fifty years, only sixteen of them can be classed as "great" hurricanes both as to intensity and diameter, and of these sixteen only two, both in southern Florida, have occurred within the last ten years.

A study of damage resulting from hurricanes shows that, aside from damage from high tides in low coastal sections and along large and shallow lakes, damage by hurricane winds to buildings constructed along or near the Gulf and South Atlantic coasts can be minimized if the walls and framework of buildings are sufficiently strong to withstand the wind pressure when the wind attains a velocity of from 100 to 125 miles an hour. It has been the experience of all Weather Bureau officials who have investigated the amounts and kinds of damage from hurricanes that even well-constructed frame buildings do not, as a rule, suffer more than relatively minor damage. Few well-constructed buildings collapse and either kill or endanger the lives of occupants, except possibly in the greatest of hurricanes. Inasmuch as the estimated property loss from hurricanes on the Gulf coast, including Florida, during the last fifteen years amounts to more than $150,000,000, and since a considerable part of this loss was due to wind damage alone, the desirability of sturdy building construction in that area is quite evident.

PSYCHOLOGY AND HIGHWAY SAFETY

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THE science of psychology, in other words the science of human behavior, is being drawn upon to help meet a national emergency, the emergency created by the rising tide of automobile accidents.

You do not need to be reminded how serious the situation is. In 1929 the fatalities increased 10 per cent. Every morning paper brings distressing evidence of the need for better ways of preventing accidents. Based on the latest figures of the National Safety Council, I estimate that in this country about eighty-five people are being killed every day in street and highway accidents, and more than twenty times that number hurt.[1]

The life insurance companies tell me that the automobile disease, as it may be called, now ranks along with tuberculosis among the ten major causes of death. For children five to fifteen years of age it heads the list. More of these boys and girls were killed last year by automobiles than died of diphtheria, scarlet fever or typhoid. The very little children not yet in school are specially liable to injury, also people over fifty years of age. But none of us, old or young, whether we drive, ride or walk, are wholly free from this danger. Probably among all of you who are listening there is scarcely one who is not now thinking of some relative or friend who has suffered in an automobile crash.

Until due account is taken of both the physical and the mental circumstances related to accidents, it is not possible to make a comprehensive workable plan for accident reduction.

The external physical conditions that lead to accidents have as a matter of fact

[1] The reported deaths from motor vehicle accidents this year to September 1 indicate a 6 per cent. increase over 1929. At this rate, the 1930 fatalities will be in excess of 33,000.

received more intensive study by safety engineers than the human or personal factors. Traffic engineers have given a great deal of thought to problems of routing, to devising better systems of traffic control. Civil engineers have been improving the design of highways in the interest of safety, and constructing road surfaces on which skidding is less liable to occur. Mechanical engineers have greatly improved the design of the automobile so that brakes and steering-gear are more dependable and more easily controlled. Others have been inventing road signs and markings that can be more quickly and easily seen, by day and by night. The various mechanical and material aspects of the safety problem, then, have had the attention of some of the ablest engineering brains.

But what of the driver at the wheel? He too is a problem. As a matter of fact only a minor part of the accidents that happen are primarily due to failure of brakes, tires, steering-gear, headlights, slippery road surfaces or other physical conditions. Indeed, we estimated at the time of the second Hoover Conference on Street and Highway Safety in 1926 that between three quarters and nine tenths of all the accidents are traceable mainly to failure of the human factor. This is why industrial psychology has been called upon to make its contribution to highway safety.

Industrial psychologists have been studying the causes of these automobile accidents, especially mental causes. When a policeman sends in a report of a collision he sometimes puts it down as caused by inattention. One of the state motor vehicle bureaus has classified as due to inattention nearly one fifth of all the accidents reported. The psychologist asks, why was this person inattentive? Why was his mind wandering at the crucial moment? Was it a fair companion who distracted him? Was it because of an illuminated advertisement that caught his eye? Or was the source of absent-mindedness within himself—some hidden worry or anxiety? You know how hard it is to concentrate on what you are doing when your mind is preoccupied with financial difficulties or a love affair or trouble at home or an argument you have had with the boss. Under such mental conditions you are more apt to be surprised by a sudden emergency or upset by the excited remark of a back-seat driver. You suddenly turn right when you meant to turn left, and before you know it you have ditched the car. The more we learn about these mental hazards of driving, the better prepared we are to overcome them.

Many other psychological factors have to be reckoned with also. There are too many people who can pass an examination for competence in operating a car, but who later develop wrong habits of driving, or who get rash or careless or a bit intoxicated, or are in too much of a hurry or who want to show off. Then they have accidents. Perhaps they get overtired, or develop a nervous headache or some other illness which makes it harder to keep concentrated on the job of driving—with disastrous results.

My wise friend Bill Pfouts used to say, "The best safety device is located above the neck—about four inches above the neck." What has psychology done toward improving the operation of this most essential safety device?

The story would be long, if all were told. Distinguished psychologists in Europe and America have delved into the mysteries of human motive and habit, attention and distraction, visual acuity, reaction-time, susceptibility to fatigue, self-control and other aspects of human nature in search of obscure causes of proneness to accident and ways of removing them. Our colleagues in England have studied the relation of accidents to proficiency and to various differences of ability and personality. They early proved that accidents do not

just happen; they are not distributed according to the laws of chance. In studying records of accidents among factory employees, street-car motormen, automobilists and bus drivers we too have found that more than half the accidents occur to a relatively small proportion of the men. These are the accident-prone people, the repeaters. They have had the same training and supervision as the others. They see the same posters and take part in the same safety drives. Yet they continue to have accidents. So the problem of the industrial psychologist is clearly that of developing the most effective ways of studying these accident-prone persons and helping them to overcome their particular tendencies.

Accidents have been largely reduced where this psychological approach has been added to the more familiar forms of effective safety effort—on the street-railways and bus services of Boston, for example, where we have had opportunity during the past three years to cooperate with the management, study the problem in detail and observe the benefits to employees and public which have followed the use of the measures recommended. There, under my direction, continuing studies have been carried forward and practical aids developed and installed. Collision accidents have been reduced one half. The Anthony N. Brady Memorial Gold Medal was recently awarded to this company for the best record in accident reduction of any street-railway system in the country. Men and management are proud of their fine accomplishment, and good-will toward the road has been increased among the car riders and pedestrians of the large metropolitan district which it serves. The financial saving, through reduction in deaths, personal injuries and property damages, in 1929 exceeded $300,000. The application of industrial psychology to accident prevention has its economic as well as its humanitarian values.

One of the important points we have been investigating is the relation between slight mishaps and serious accidents. Wherever adequate records have been made available, we have found that those people who have a large number of minor accidents are on the average more apt to meet with a serious disaster some day. The problem is how best to discover these accident-prone men and help them to become skilful, safe operators before they meet with a bad accident. Nearly all these men are just as anxious to avoid accidents as any one. They do not know why they so often have "tough luck" as they call it. It is our duty to find out why they have such bad luck and then help them to develop correct habits and right mental attitudes so that they can more surely avoid accidents. This has been done with marked success. The resulting decrease in accidents has been brought about, I am proud to say, not by discharging the men with bad accident records and hiring others, but by helping these men to change their ways and become competent, careful drivers. What these men have done, others can do. I believe you will agree that a large majority of the automobile accidents happening to-day are really quite unnecessary.

I am often asked whether men or women make the better drivers. Women have fewer accidents than men, but this is not because they are better drivers on the average. It is because they do not do so much driving. When men and women have a chance to operate the same kind of vehicle over the same streets for the same number of hours a day, the women have about three times as many collisions as the men. This fact came to light in a study of men and women taxicab operators in Philadelphia, published in the *Personnel Journal* for February, 1929.

Women and girls have the advantage over men in at least one regard. Not

nearly so many of them are color-blind. They rarely have any trouble in telling red from green. But the latest researches show that about one man in twelve is born with this peculiarity of the retina. When a driver runs by a red light it is sometimes because his eyes are defective in this respect. If such a person goes to the garden to get some tomatoes, he is apt to come back with a few green ones in his basket along with the red ones. All tomatoes look alike to him. When he is driving an automobile he naturally tends to confuse red and green signals. Curiously enough, many such drivers are quite unaware that they have this handicap.

So psychologists have cooperated with safety engineers in standardizing the colors to be used in highway signals. They have found the particular hues of red and green that are least apt to be confused, even by drivers with this color defect. They have also undertaken to arrange these signals so that even the color-blind eye can readily tell them apart by their shape, position or number.

Color-blindness and other visual defects, however, are not among the major causes of highway accidents. Neither is slow reaction-time as serious a matter as is sometimes supposed. Variability of reaction-time is a better indicator of proneness to accident. A habitually narrow, focalized attention is thought to be a handicap, since a driver needs a certain dispersion of attention; he needs to know the total situation, to be aware of what is happening on both sides of him as well as directly in front. He also needs imagination enough to anticipate what the other driver is going to do next, or the little child playing beside the road. Some operators are handicapped by excessive perseveration, the tendency to keep right on doing whatever one has begun to do. Others are hard of hearing. One of my friends has to drive with only one arm. But whatever a person's natural handicaps may be, the amazing thing is his capacity to compensate for these defects. We have been astonished again and again at the ability of all sorts of people to acquire the knowledge and the skills necessary for operating an automobile safely. Even the very dull, with no more intelligence than a high-grade moron, can do it, if they are emotionally stable. The capacity of people to learn is very great, provided the training they receive is properly adapted to their individual personal needs.

But until a driver can demonstrate that he has acquired these essential abilities he ought not to be allowed on busy streets. This is why some states require examinations for driver's license.

The driver's examination ordinarily includes a practical road test, so that the examining officer has a chance to observe how skilfully or awkwardly you can handle a car in traffic. Some examinations include tests of hearing and eyesight. You are also examined either orally or in writing to see how well you know the laws of the road, the ordinary courtesies and customs of driving and the state and local regulations, including what to do about reporting any accident that may happen.

These examinations have proved to be of real value. No good driver need be afraid of taking such a practical test; and if a driver is not competent, then he is not wanted on the road, blocking traffic, making wrong turns or doing the foolish thing in an emergency. You know there was an alarming increase in the number of people killed by automobiles last year. The total exceeded 31,500. But the increase was mostly confined to those states which do not yet have a well-administered examination for driver's license. If you live in one of those states, your chances of being killed or hurt are unnecessarily high. Would it not be a good idea for you to write or talk to your representative in

the state legislature, and ask him how much longer you must wait before you too have the benefit of a good driver's license law?

The psychologists who have been studying the problem of preventing accidents are thoroughly in sympathy with the movement for drivers' examinations. Indeed, they have done a good deal to improve the methods of examination—to make them more practical, more thorough and at the same time more simple to administer. For example, twelve years ago they developed and standardized for the United States Army the practical tests for truck drivers and chauffeurs which have ever since served as model examinations. At the present time, psychologists and state motor vehicle administrators are cooperating to make such examination methods still more practical and useful.

Sifting out the most incompetent drivers by means of license examinations is, however, only one step toward accident reduction. Even more valuable is the special examination of the driver with a bad record who is brought before the authorities for a hearing to see whether his license should not be revoked. This examination aims to go to the root of the matter, to find out whether the man's unsafe driving is due mainly to his constitutional slowness or lack of muscular control; or to his ignorance, his lack of practice, his wrong attitude toward other drivers; or to his health, his emotional instability or some other complicating cause. Then a decision is reached as to whether he is probably capable of becoming a safe driver; if so, he can be started on the right road to that destination.

Policemen, judges of the traffic courts, motor vehicle administrators and public all share this responsibility for helping the dangerous automobilist to change his behavior for the better.

Industrial psychology has not yet solved all the problems of accident susceptibility, nor discovered all the effective means of curing drivers of their dangerous ways. Indeed, scarcely more than a good beginning has been made. I believe you will agree, however, that this psychological approach to the problem is essentially sound. You can put it to the test by observing your own habits of driving, your own thoughts and motives while at the wheel, your attitudes toward other drivers and toward pedestrians and your behavior when a sudden emergency arises. This is a fascinating game. Who knows but what it may help you sometime to avoid one of these all too common automobile fatalities!

HEREDITY AND ENVIRONMENT

By Dr. ALBERT F. BLAKESLEE

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THIS last summer a newspaper reporter asked me to predict the future of the Lindbergh baby from the standpoint of its inheritance. The reporter seemed to think a student of heredity ought to be able to tell what a child will amount to if he knows what its parents have accomplished. I declined, however, to be a fortune-teller and give a detailed horoscope of the infant. No doubt I was expected to say that the child will become a great flier like his father. If the child should spend many of his future hours in the air, as he doubtless will, would this be chiefly because he has inherited his father's

capacity to learn aviation or chiefly because he is brought up in aviation surroundings? In other words, would flying in his case be due to heredity or to environment? As a matter of fact it would probably be due to both. The capacity to learn to coordinate body and mind necessary to one who guides an airplane is undoubtedly inherited. The flyer Lindbergh inherited such capacity from his ancestors, although his ancestors never had an opportunity to show this capacity in actual flying. They lacked an aviation environment. On the other hand, whatever our heredity, probably most of us who are listening in will fly before we die, at least as passengers, and many of us hope to make flights with our own wings afterwards.

If I had made the prediction that Baby Lindbergh would become a Congressman I would have had some justification from both his heredity and his environment. One grandfather was a Congressman and the other grandfather seems likely soon to be a Senator. His environment is no less propitious than his heredity. He was born in a household which was conspicuous in the political world, and he is likely to be brought up in an atmosphere in which political ideals are familiar.

The future of Baby Lindbergh, and of every other baby, will depend upon the potentialities with which it is born and the development of these potentialities by means of the surroundings to which it is subjected. In other words, the future of the child is determined by the interaction of the two major factors controlling life—heredity and environment.

The biologist does not mean by heredity the so-called inheritance of money or of antique furniture. He means by heredity what the individual receives from his ancestors not as visible heirlooms but as capacities to develop and respond in definite ways under different surroundings. The surroundings or opportunities to which the individual responds form his environment.

Man is subject to the same biological laws as other animals. But man is not a convenient animal for experimentation. The laws of life in consequence have been discovered chiefly through experiments with other animals and with plants. You would probably all agree, from your own observations, that no two human beings are exactly alike. Probably the same is true of at least the higher plants and animals. I used to take my students to an apple-tree and ask them to find two leaves that were exactly alike, that is, two leaves which when placed one over the other would correspond in size, shape, teeth on the margins and veining. Each year the class would start with the idea that it would be easy to find two identical specimens of such a relatively simple structure as a leaf. They all finally would agree, however, that probably in no kind of tree could two leaves be found which were exactly alike. It might be pointed out that the peculiarities which enable one to distinguish the leaf of an apple from that of another kind of tree, such as a peach, for example, are due to differences in its parentage or in other words to its heredity. The differences between the leaves on the same tree are due to differences in environment during their development, such as differences in light, in position on the twig, in vigor of growth, etc.

During the last thirty years much has been learned about the cause of differences between living things. Environmental influences such as light, temperature and food, including the vitamins, have been found to affect profoundly the structure and reactions of plants and animals. A mechanism of inheritance has been discovered. It is now known that characteristics are not transmitted as such from parent to offspring. What is inherited are rather hereditary units which interact and

give the plant or animal the power to respond in a certain way to a given kind of environment.

If the environment is changed, the plant or animal may respond in a different way without changing its heredity. Thus if we dislike pink-flowered hydrangeas we can make their flowers blue by adding iron salts to the soil. But their offspring, whether they came from pink or from blue flowers, would be alike. In both cases they would inherit the same power to respond to the amount of available iron in the soil, and their color would depend upon how much iron they were able to absorb.

An example may be given from animals. The so-called Himalayan breed of rabbits is white except for its extremities which are black. It was discovered by experiment that if the temperature of the skin were lowered, the hairs that grew out would be black. This particular breed of rabbit inherits the capacity to develop white hairs if the temperature is high and black hairs if the temperature is low. Whether the animal had white or black hairs, however, would make no difference in the color of its offspring — no more than would dyeing the hair of the mother with whatever cosmetic humans use to make gray hair look young. It is not color of hair as such that is inherited in this breed of rabbit but rather the power to produce white or black hairs in response to a high or low temperature environment. Most breeds of domestic rabbits do not inherit the power to respond to temperature differences in their environment by a change of coat color. They are either white or pigmented at all temperatures.

It is often difficult to decide whether to attribute a difference between two forms primarily to heredity or to environment. I sowed two batches of seed from a single ear of corn. One batch I planted in hills far apart. The variety had inherited the power to grow tall under favorable conditions and the plants reached a height of ten or twelve feet. The other batch of seed I planted close together. They grew only three or four feet high. Shortness in this case might be attributed to environmental factors of crowding, since the heredity was the same. Another variety of corn, called the Tom Thumb variety, was planted at the same time in hills far apart and in the same kind of soil. It was a dwarf variety and grew only three or four feet high. In comparison with the variety of tall corn under similar environmental conditions, the shortness of the Tom Thumb variety might be attributed to heredity, since the environment was the same. Thus it will be seen that both heredity and environment may bring about similar effects. The shortness of growth in corn is an example.

Now what about the effects of heredity and environment upon the next generation? Seeds from the Tom Thumb dwarf variety gave dwarf plants the next season, as might have been expected from the heredity of this variety.

I planted also seeds from the corn plants which had been dwarfed by crowding, alongside of seeds from the tall corn which had been grown far apart. There was no difference in the height of the offspring that resulted. The dwarfing due to the environment had not been inherited.

Environment and heredity as causes are fundamentally different. Environment works upon the individual and its effects are transient. Heredity works upon the offspring and its effects are enduring, from generation to generation. Inherent characteristics only are transmitted. It has been generally assumed, however, that characters acquired as a response to a good or to a bad environment are handed on to the next generation—that a man's college education, for example, will affect the

mind of his newly born children. Increasing knowledge has failed to support this assumption. So-called acquired characters seemingly can not be handed on to one's descendants.

It follows, therefore, that no amount of exercise of mental or of physical powers of parents can directly affect their children. A man blinded at birth would have no opportunity to practice the painter's art, however great the artistic gifts with which he was born. Yet his children would inherit no less artistic power than if he had retained full sight and become famous by his artistic production. This is not to say that in both cases the chances would be equal of his children developing into recognized artists. On the contrary, any artistic education of the father would be handed down by example to the children and this environment would afford them a better opportunity of recognizing and developing any artistic talents they might have inherited.

We start life like a photographic plate which has been exposed. There is a potential image ready for development, which corresponds to the heredity. Chemical solutions in the hands of the photographer furnish an environment which reveals the lines already impressed upon the negative. Differences in this environment brought about by changes in the manner of developing the negative may alter the appearance of the finished picture. And yet the development can bring nothing new into the picture. Its outlines were foreordained at the moment the sensitized plate was exposed in the camera. After we are born, we can not change our heredity, though we can change our environment.

The facts that have been presented may appear familiar and the distinctions drawn trivial. But the distinctions lie at the base of all efforts for human betterment and are far from trivial in their significance.

The distinction between heredity and environment and their relative influence should be carefully estimated in any rational campaign for permanent social improvement. Our ideals and practice in social and religious justice and in charity and education are fundamentally dependent upon our estimate of the relative value of these two factors. If one is born with inherent criminalistic traits, for example, the environment may be in no way responsible, and can thus be neglected. Shall we, however, merely punish the individual or shall we attack the real cause of the crime—his heredity? On the other hand, if the criminal is made such by his environment, shall we confine our attention to the criminal and neglect the environment which has made him a criminal?

In this brief talk I have tried to show that what every living being is and does is dependent upon the constantly interacting factors of heredity and environment. Every thought and act of our lives is influenced by these two factors.

The question is often asked if a particular type of personality is due to heredity or environment, as if it were due to one and not to the other. The question finds expression in the phrase nature *or* nurture. The phrase should read rather nature *and* nurture, or, in the words of the title of my talk—heredity *and* environment.

INVESTIGATING TETANUS (LOCKJAW)

By GEORGE E. COLEMAN
UNIVERSITY OF CALIFORNIA

THIS is an era not only of outstanding scientific achievement but also of educational activities sponsored and aided for the benefit of the people by scientists themselves. Particularly is this true of the medical sciences, and fortunately so, for in no other field is it possible for an interested and suffering public, avid for reliable information concerning health and disease, to be so led astray. Radio broadcasting by the American Medical Association, educational medical films, lectures by hospital staffs, life insurance pamphlets and authoritative books and articles in magazines and newspapers are all rapidly opening for the masses the gates to a field hitherto closed and more or less wilfully hedged with secretiveness. The public is at last and truly becoming scientifically minded and therefore able to realize more and more some of the difficulties inherent in the solution of the most baffling medical problems. The details of the investigations of some of those successfully solved would, if described in the vein of Fabre's works on insects, thrill the reader like a best seller among detective stories, for they too deal with life and mystery.

While cancer continues to take its enormous annual toll of human lives there is another disease—tetanus, popularly called lockjaw—which is fortunately far less prevalent but equally or even more deadly. This disease possesses many fascinating problems for the medical investigator. These hinge largely upon the many peculiar properties of the germ of tetanus (Clostridium tetani).

BACTERIOLOGY

Let us visualize the microscopic picture of this bacillus when it is grown in pure culture and has reached the stage of sporulation. All one sees is innumerable short, thin rods, many of them with a perfectly spherical ring or ball (spore) at one end. These are called drum-stick forms. There are also seen many free spores which have lost their bacillary attachment. This organism grows and produces its fatal toxin or poison only in the absence of free oxygen. It is therefore called an anaerobe. The bacillus also forms a spore—a resting stage or form of resistance which enables it to perpetuate the species under adverse conditions, such as drying or lack of proper food, which would permit it to continue to propagate by division of its cells. Tetanus is not a contagious disease but develops only after deep punctured or contused wounds are received in which dirt or something else contaminated with tetanus spores may have entered.

There are several other species of spore-bearing anaerobes which cause fatal wound infections, particularly that of gas-gangrene, but none forming spores more resistant to heat and antiseptics. Nor do we know of any which produces a more potent toxin or one which acts on the nervous system in such an insidious or peculiar way.

THE POISON

In passing I should mention the anaerobe Clostridium botulinum which produces a powerful poison in canned and preserved foods and which has been the cause of many fatal outbreaks of botulism. This organism perhaps shares with the tetanus bacillus the doubtful honor of producing one of the most deadly poisons of any living thing. We know little quantitatively of the toxic

secretions of insects, but judging from the effects of the minute quantities injected when they sting, it is possible that some of them are as potent as that of the tetanus bacillus. To give an idea of the amount of poison produced by a very toxic strain of the tetanus bacillus when grown in a favorable liquid medium—if one were to mix one drop of the culture with ten quarts of water, twenty drops (approximately one cubic centimeter) of this mixture injected under the skin of a mouse would kill it within four days. This amount would be called the minimum lethal dose (M.L.D.) of this culture and contains about one ten-thousandth of the original drop. It would also be equal to one cubic centimeter of the culture diluted two hundred thousand times. We know the relative susceptibility to tetanus toxin of the various animal species and birds used in bacteriological research but practically nothing quantitatively regarding man. It has been estimated from clinical data that, excepting the horse, he is probably the most susceptible of all.

The resistance of the spores to dry heat and boiling varies according to the particular strain and other conditions. Certain strains will resist a boiling temperature for over an hour, while those of the botulinus organism will resist boiling for five hours. Both are also extremely resistant to antiseptics, and certain strains of tetanus spores may survive over one hour in dilute commercial lysol.

Another feature which adds to our difficulties in the study of Cl. tetani is its very special nutritional requirements. Most material to be examined (stools, earth, wound exudates) are teeming with other bacteria in vastly greater numbers than the tetanus organisms. These outgrow and overgrow the latter in culture, which makes isolation and proof that the material contains tetanus spores or bacilli very difficult. Then, too, the metabolic products produced in culture by these other organisms often destroy what small amount of toxin may be formed by the growth of the tetanus bacilli, if present, and so mouse inoculation is often of no avail in proving the presence of tetanus toxin.

So much (perhaps too much) for the bacteriology of tetanus. Before considering the natural and experimental disease it will be necessary to touch briefly upon the question of immunity.

Serology

If an animal be injected two or more times with the bacilli, washed to free them from their toxin, within about three weeks its serum, separated from the clotted blood, will acquire a peculiar property. If a few drops of this serum, even diluted many thousand times, be added to a homogeneous saline suspension of the same strain of bacilli which was injected, in a short time the bacilli will all clump together. No amount of shaking will separate them. This is called agglutination, and is a very reliable and constantly used method for the diagnosis of certain diseases as well as for estimating the relationship which may exist between strains of the same bacterial species. Formerly it was supposed that all tetanus strains were alike. Thanks to an intensive study of this phenomenon begun during the war, we are now able to differentiate nine types of this species. Five were discovered in Europe, two in China and two at the University of California. All types have the same morphological and cultural characteristics as far as can be determined.

Immunity

Nearly every one now knows something about antiserums or antitoxins since the immunization of children against diphtheria has proved such a boon. Briefly, the serum of an animal will after a time acquire the property of neutralizing, either in the test-tube or in

the body, the toxin of certain species (tetanus, diphtheria, botulinus, scarlet fever) after the animal has been injected with several gradually increased doses of any one of these toxins. This reaction is specific only for the kind of toxin injected. This applies to snake venoms and other substances as well. For purposes of immunization of both man and animals, formerly tetanus toxin was partially neutralized with antitoxin before being injected, but a newer method is now practiced which avoids the use of antitoxic serum and includes a modification of the toxin by formalin. This chemical destroys the very toxic principle of the toxin but leaves intact its immunizing bodies. The use of this latter product with human beings avoids possible serum sickness. It is called anatoxin, while the other is called toxin-antitoxin. Antitoxic serums from horses immunized against tetanus are now concentrated and standardized so that one cubic centimeter (about twenty drops) of a potent serum is often obtained which will neutralize one to two million guinea-pig minimum lethal doses of toxin. This is the product which is now used to prevent and to attempt the cure of developed tetanus in the wounded.

By the injection of formalinized bacteria and spores into animals one can produce an antibacterial or an antispore serum which has distinct bactericidal or sporocidal properties—at least in the test-tube. It is the hope of research to prevent the growth of tetanus spores and bacilli in the body by this means. As yet no clear-cut results have been obtained with tetanus, but recently English workers have obtained some very promising leads with other wound-infecting anaerobes. The question as to whether these sera obtained with one type of tetanus bacillus will be effective *in vivo* against all other types is still undetermined. This is perhaps the most important problem before us to-day.

GERMS OF TETANUS

ABOVE: COLONIES OF CL. TETANI GROWING IN AGAR. BELOW: TETANUS SPORES FROM A CULTURE MAGNIFIED 1,300 TIMES.

TETANUS CARRIERS

About 25 per cent. of the normal population of the San Francisco Bay region have tetanus spores in their intestinal tract. It has been claimed by one worker that "carriers," to a great extent, are immune to tetanus, but neither the writer nor any one else as far as the literature has revealed has ever found a trace of antitoxin in normal human sera whether of carriers or non-carriers. Some normal human sera, however, very slightly diluted will agglutinate certain types of tetanus bacilli—some, in fact, several types in the same individual. It has been pretty well dem-

onstrated that immunity to tetanus is a humoral property and is not something inherent in the cells of the body. That is why we estimate the degree of immunity by the antitoxin content of the serum. It is possible that the carrier state is transient in any given individual. One can feed millions of tetanus spores to guinea-pigs several times in one week, and often two or three weeks later none can be demonstrated in their stools. These studies are important because in our attempts to produce the most effective preventive or curative sera we must follow the agglutination, sporocidal, bactericidal and antitoxin reactions of the serum as the immunization proceeds. This is in order to elucidate the question of the possible effect of the carrier state in immunity to tetanus.

A GUINEA PIG WITH TETANUS

AN INDIVIDUAL INFECTED WITH TETANUS SPORES IN LEFT HIND LEG. NOTE THE TWISTED SPINE.

THE DISEASE

Tetanus spores are usually more numerous in soil that has been fertilized over long periods with manure. That is why during the World War such a large number of the wounded in France developed tetanus. The writer was at the Pasteur Institute at the beginning of the war, and as the Germans neared Paris the first thought of the French was to save their precious immunized horses and to remove them from the city. The serum, however, was soon exhausted and tetanus took a heavy toll. Later on when hundreds of horses became immunized in France and England every severely wounded soldier received one or several injections of antiserum to neutralize any tetanus toxin which later might be developed in the wound. Thousands of men were thus saved from this dread disease. Others also, though receiving preventive doses of serum, developed the disease and some died, although serum treatment in enormous doses was continued. Under just what conditions does our preventive serum protect and all too often even in enormous quantities fail to cure the developed disease? That is what we are trying to find out. The layman has no idea of the years of study by numerous investigators all over the world or of the thousands of mice and guinea-pigs sacrificed to try to solve this and other riddles of medical science.

The tetanus bacilli developed from spores for the most part remain in the wound and produce their toxin there. Their development is aided by the oxygen-consuming ability of other bacterial contaminants (called aerobes), by the tissue debilitating substances produced by them and by other anaerobes and by a host of other factors only partially elucidated. The question of whether tetanus spores will develop in a wound is apparently determined not so much by the ability of the white blood

corpuscles to destroy them as by the degree of oxygen tension which may exist there. Where pus and extensively devitalized tissue is present this may possibly fall to zero and the spores then develop.

As the period of incubation of tetanus in the wounded may vary from three or four days to six months or more, in fact long after the wound has completely healed, the mechanism of tetanus infection becomes one of the greatest complexity. The toxin produced when development begins gradually increases as the bacilli multiply. This creeps up the motor nerve endings almost as a root-fiber sucks up moisture, and beginning in the affected part slowly paralyzes the entire motor nervous system. When once this combination of nerve substance and toxin takes place, no known medical procedure can separate them. Nor can the continued absorption of the toxin be prevented except (1) by the neutralization of uncombined toxin which, when rapidly produced, also enters the blood stream and may be absorbed by nerve endings all over the body, and (2) by the prevention of the further development of bacilli in the wound. In developed tetanus these continued injections of large quantities of antitoxin undoubtedly take care of much of the free toxin remaining unabsorbed by the nerves, but unless the development of the bacilli can be controlled there is a limit to the endurance of the sufferer, and he dies, especially if treatment is not begun early and continued vigorously.

Experimentally our first endeavor is to reproduce the natural infection in animals with counted spores. Even when injected after severely devitalizing the muscle with other bacteria or chemicals, until recently several hundred million spores were required to infect guinea-pigs. A method has now been perfected at the University of California which enables us to produce tetanus with only ten to fifty spores. With this number in all probability natural infection is more nearly simulated, toxin production reduced and more accurate results obtained in the study of preventive or curative serums.

The particular questions involved in prevention are those concerning the prophylactic serum. The toxin of all types of tetanus bacilli is neutralized by the serum produced by immunizing animals with the toxin of any one of the nine given types thus far discovered. But this serum, as now prepared, probably plays a negligible part in actually preventing the germination of tetanus spores in a wound. Other factors determine this, and the antiserum merely neutralizes the toxin if it is not too rapidly produced. In the meantime the natural defensive mechanism of the body may stop the bacilli from growing. When this does not occur may it not be possible more surely to prevent or to cure developed tetanus if our present antitoxic serum can be made to include spore or bacterial growth-inhibiting substances? This is another one of our major problems. Some eminent English investigators feel that the serologists have done all they can and that the physiologists will now have to continue the work. Many physiological investigations with drugs are in progress with a view to the reinforcement of the natural defenses of the body in its resistance to the infection as well as to the effect of the absorbed toxin, but the complete solution is not yet in sight.

It is thus that scientists work and struggle to expose the hidden secrets of nature and to control her processes. Meanwhile humanity waits, but with justifiable hope. Its part is to encourage science by its interest and appreciation and to furnish the necessary material aid that greater numbers of competent scientists may be highly trained and thus hasten the solution of our complicated problems.

THE PROGRESS OF SCIENCE

THE FIFTH INTERNATIONAL BOTANICAL CONGRESS

THE Fifth International Botanical Congress at Cambridge, England, under the presidency of Professor A. C. Steward, of Cambridge University, August 16 to 23, 1930, was initiated by a formal reception to delegates and members of their families at the Imperial Institute in London on Friday evening, August 13. The following morning a special train took the delegates to Cambridge, where the congress officially convened on Monday morning, August 16.

The presidents of the sections were as follows: Bacteriology, Professor R. E. Buchanan, Iowa State College; Phytogeography and Ecology, Professor H. C. Cowles, University of Chicago; Genetics and Cytology, Professor O. Rosenberg, Botaniska Institutet, Stockholm; Morphology and Anatomy, Professor J. C. Schoute, Gröningen; Mycology and Plant Pathology, Professor L. R. Jones, University of Wisconsin; Plant Physiology, Dr. F. F. Blackman, Cambridge University; Paleobotany, Dr. D. H. Scott, Basingstoke; Taxonomy and Nomenclature, Professor L. Diels, Berlin.

The scope of this paper does not permit a detailed consideration of the numerous and important papers that were presented before the various sections of the congress. Abstracts of these communications were published in advance of the congress, forming a book of 327 pages, copies of which were supplied to all delegates. Provision was made for the ultimate publication of the full proceedings, and the papers presented are now in the hands of the editorial committee.

The executive committee of British botanists, charged with making detailed arrangements for the congress, is to be congratulated on the excellence of their program and the manner in which the numerous items were handled. The very efficiency of this committee added greatly to the pleasure and convenience of the numerous overseas delegates.

All meetings were held in the lecture rooms and laboratories of Cambridge University, and the majority of the delegates were housed in the various colleges of the university. Because of the most attractive surroundings, the ample space for conferences, exhibits, etc., and the close proximity of the various buildings, the numerous delegates who were fortunately able to attend the congress will long remember the pleasant associations there formed, and all keenly appreciated the wholehearted hospitality and the numerous courtesies extended to them during their stay in Cambridge and in England.

Various important excursions were arranged for visiting delegates before, during and after the congress. These included a pilgrimage to Halesworth Church to attend the dedication ceremonies of a memorial tablet to Sir Joseph Dalton Hooker, born at Halesworth, and Sir William Joseph Hooker; to the English beech woods in West Sussex, Cheltern Hills, Cotswold Hills and the Wye Valley; to Blakeney Point and Scolt Head to inspect their maritime vegetation; to Wicken Fen to examine the swamp vegetation; special field excursions for plant pathologists; excursions to the Rothamsted Experimental Station, Harpenden; the John Innes Horticultural Institution, Weston; Messrs. Sutton and Sons Seed Establishment and Trial Grounds, Read-

EXECUTIVE COMMITTEE AND OFFICERS OF THE BOTANICAL CONGRESS

LEFT TO RIGHT, FRONT ROW: H. HARMS, G. W. E. LODER, B. NÉMEC, F. A. F. C. WENT, C. SCHRÖTER, E. D. MERRILL, L. DIELS, A. C. SEWARD, G. R. WIELAND, H. C. COWLES, J. C. SCHOUTE, K. DOMIN, R. MAIRE, A. B. RENDLE, J. BRIQUET, DAVID PRAIN. SECOND ROW: T. F. CHIPP, O. STAPF, A. RENTER, W. GOTHAN, W. J. JONGMANS, NELLIE CARTER, E. RÜBEL, T. A. SPRAGUE, R. E. FRIES, O. ROSENBERG, N. E. SVEDELIUS, HELEN GWYNNE-VAUGHAN, A. PASCHER, R. R. GATES, S. ORLA-JENSEN, R. E. BUCHANAN, E. R. SAUNDERS. THIRD ROW: F. T. BROOKS, M. KORCZEWSKI, W. SZAFER, F. F. MATURANA, F. F. BLACKMANN, W. L. BALLS, P. BEETRAND, B. SAHNI, T. G. HALLE, A. W. HILL, F. G. D. MURRAY, W. N. EDWARDS, O. WINGE, J. RAMSBOTTOM. BACK ROW: H. O. JUEL, F. E. FRITSCH, A. D. COTTON, R. KRAUSEL, W. STILES, G. E. BRIGGS, A. G. TANSLEY, V. H. BLACKMAN, R. PAMPANINI, H. H. THOMAS, H. GODWIN, H. GLÜCK, H. H. DIXON, F. E. LLOYD.

ing and Slough; Messrs. James Carter and Company Seed Establishment and Trial Grounds, Raynes Park. Through the courtesy of the president and council of the Royal Horticultural Society, the garden of this society at Wisley was open to delegates; the same courteous arrangement was made by the trustees of the Chelsea Physic Garden. A special exhibit of material of historic interest was arranged in the department of botany, British Museum (Natural History). At Kew a personally conducted tour through the Royal Botanic Gardens was provided, with an al fresco luncheon for visiting delegates. A special exhibition of Linneana was arranged at the rooms of the Linnean Society, Burlington House, London.

The Fourth International Botanical Congress held at Cornell University, Ithaca, New York, in 1925, had charged the 1930 congress with the difficult and complicated task of revising the International Code of Botanical Nomenclature. This was unquestionably the most important single project that came before the congress. The international committee, under the leadership of Dr. John Briquet, of Geneva, had carefully assembled and collated the data that were to be presented for discussion, these data being supplemented by a 200-page document consisting of proposals by British botanists. The "Recueil synoptique" and "Avis préalable," prepared by Dr. Briquet and his colleagues, contained all the suggested modifications of the International Code that had been submitted by botanists from all countries; in parallel columns were given the original text of the 1905 and 1910 rules, together with the suggested changes and with the recommendation of the international committee.

With these carefully and critically prepared documents, the Section on Nomenclature, under the chairmanship of Dr. E. D. Merrill, proceeded to a consideration of details. Practically all the recommendations of the central committee were approved, there being at times a very lively discussion from the floor. As discussions, amendments, motions, etc., were in English, French and German it was by no means an easy task to follow all details, although important items were interpreted by the several secretaries, Dr. Harms, of Berlin, in German, and Dr. Briquet, of Geneva, in French. During the first two days progress was relatively slow, but real progress was made on the third day by disposing of all the minor matters on a single motion, thus leaving time to consider the few really important and more or less controversial items. The proposed amendments of the International Code had to do largely with unifying, as far as possible, the divergences in this code and the so-called American Code.

Few formal votes were taken, most of the motions, after discussion, being disposed of by show of hands. In reference to important controversial matters the proportions of the vote as between yes and no were impressive. Thus, in reference to required Latin diagnoses for new species proposed from January, 1932, the formal vote was yes 371, no 24; the original spelling of generic and specific names was maintained by a vote of 342 to 21. An adoption of the rule, the contention of many American botanists, that a validly published homonym invalidates the use of the same specific name for another species was carried by yes 261, no 111; the adoption of the principle of types or standard species for genera and the confirmation and extension of the principle of *nomina generica conservanda* were carried by equally impressive majorities, the latter being practically unanimous. A proposition to make the year 1753 the begin-

ning date for binomials in all groups of plants was lost, the vote being 158 yes, 239 no.

One of the final actions of the section was to appoint a large international committee, having in its membership one or more representatives in each country, to consider interim problems in relation to the international rules, the details to be handled by a small executive committee. In addition to the appointment of the international committee to consider problems of nomenclature, other important committees were appointed, including one to standardize the terminology used in ecology; one to compile and publish an international botanical address book, and one to standardize descriptive terms used in systematic botany.

At the final plenary session of the congress, the invitation of the Netherlands to hold the next congress at Amsterdam in 1935 was accepted.

The Fifth International Botanical Congress was, unquestionably, the largest and most important gathering of its kind yet held. In excess of 50 countries were represented by about 1,200 official delegates. Naturally, Great Britain was particularly well represented, but there were about 300 delegates in attendance from the United States. Other important countries had large contingents, and most of the smaller commonwealths had representatives present. The congress was truly international in all respects, its attendance including many of the outstanding botanists of the entire world. The entire congress was pervaded by a pronounced spirit of international goodwill, which was particularly noticeable in the long and complicated discussions appertaining to the problems of nomenclature; here many divergent opinions were held, yet all present were inspired by the desire to facilitate the proceedings; each was willing to give and take; and all who took part in the discussions or who merely attended in order to familiarize themselves with the complicated questions under discussion were impressed with the desirability of coming to an understanding that would be truly international as well as acceptable to the majority of botanists in all fields of endeavor.

E. D. MERRILL

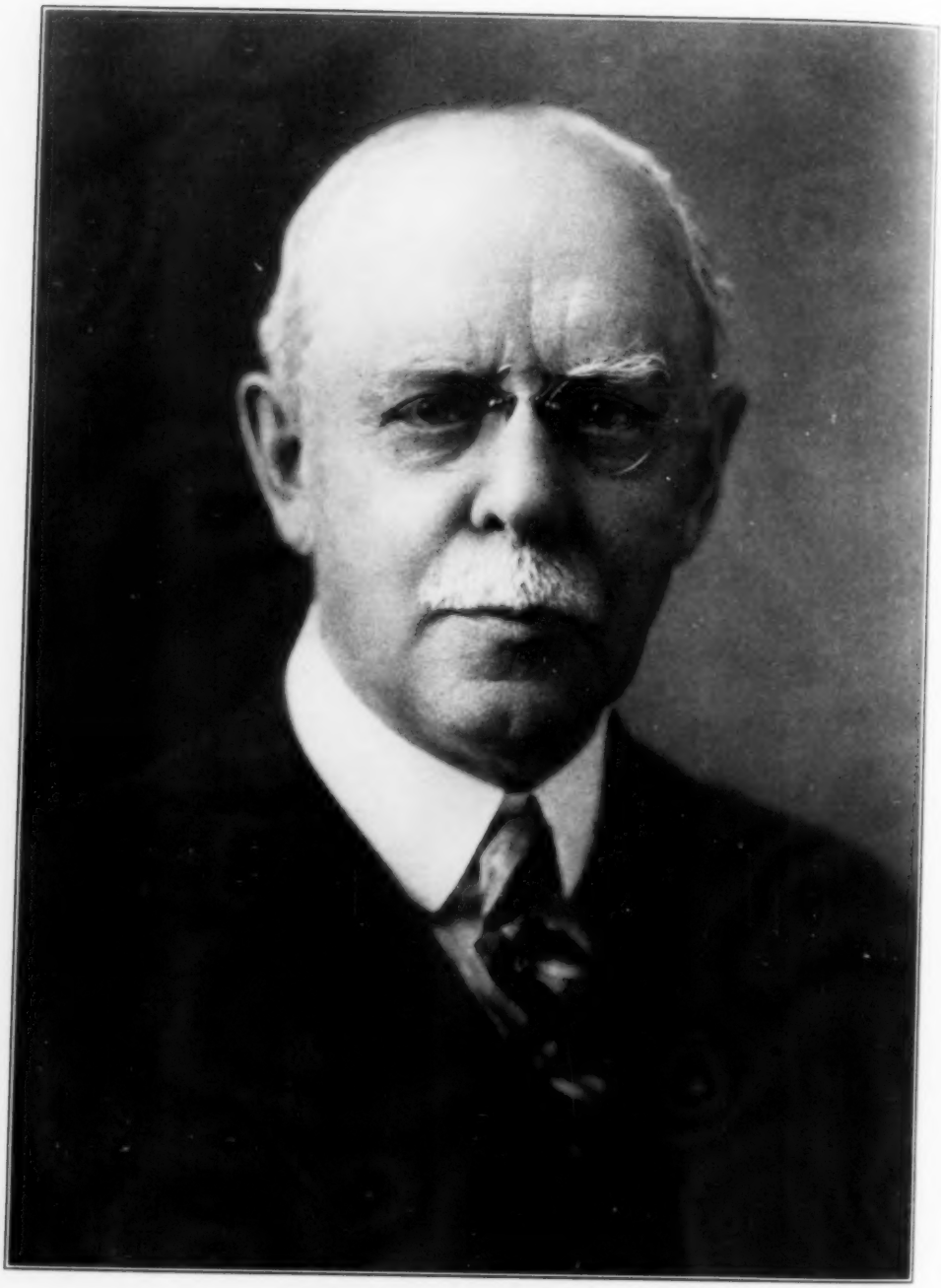
JOHN GEORGE HAGEN, S.J.

IN the quiet of the secluded little infirmary of the German College, Rome, Father John G. Hagen, S.J., piously passed to his eternal reward on the morning of September 6, 1930.

Father Hagen was born at Bergenz, Austria, March 6, 1847. Upon the completion of his early schooling, he entered the Society of Jesus and pursued his higher studies at Feldkirch, Münster and Bonn. Shortly after his ordination to the priesthood he came to the United States and was employed as professor of mathematics at Prairie du Chien, Wisconsin. Being appointed to the directorship of the Georgetown College Observatory, Washington, D. C., in 1888, he remained in this position until, in 1906, he was summoned by the Roman Pontiff, Pope Pius X, to take charge of the Vatican Observatory, Rome. With untiring devotion, increasingly difficult as his years advanced, he continued at this post until his last illness forced his retirement. Upon the occasion of his eightieth birthday, in 1927, the Holy Father, Pope Pius XI, had struck a gold medal and presented it to Father Hagen in recognition of distinguished service rendered in the field of astronomical research. Astronomy lost in the death of Father Hagen a splendid contributor and a tireless worker, and a wide circle of acquaintances a charming friend.

The "Atlas Stellarum Variabilium" may well be styled Father Hagen's lifework. Only those familiar with the study of variable stars can properly ap-

FATHER JOHN G. HAGEN, S.J.



DR. ARTHUR D. LITTLE

preciate the worth of this contribution. Zones +55 to +64 of the "Astrographic Catalogue" were completed at the Vatican Observatory, under Father Hagen's direction. Under the title "Die Veränderlichen Sterne" Father Hagen presented a historical survey of work done in the field of variable stars. During the last ten years of his life, he gave much time to the observation of cosmic clouds—especially stressing the 52 extended, diffused nebulosities of W. Herschel and Barnard's 349 dark objects. A long list of published articles splendidly evidences his wide interest and untiring zeal in his chosen profession.

Father Hagen was a member of the leading astronomical societies both here and abroad, and served, from the time of its establishment, on four of the commissions of the International Astronomical Union.

PAUL A. MCNALLY, S.J., *Director*,
Georgetown College Observatory

THE AWARD OF THE PERKIN MEDAL TO DR. LITTLE

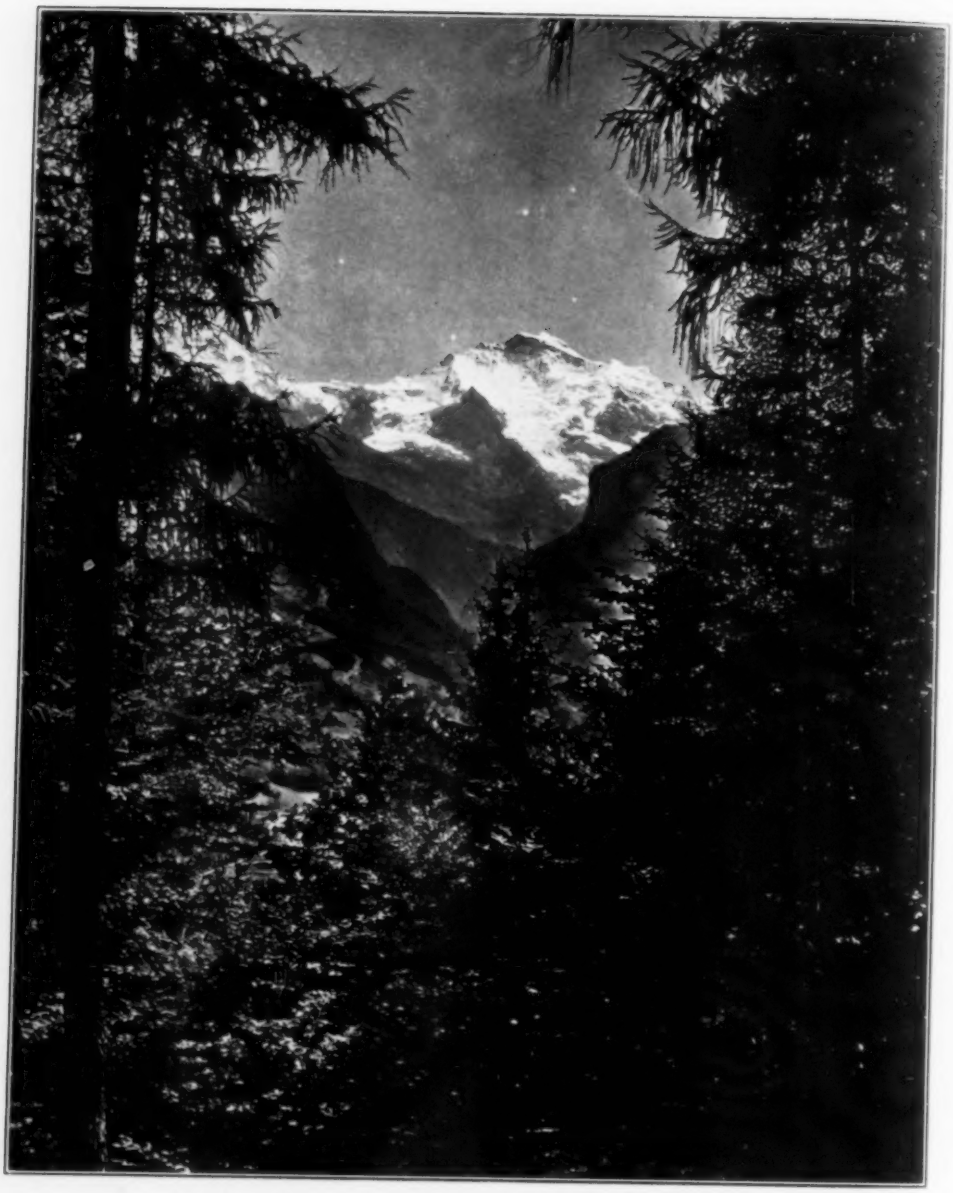
THE Perkin Medal for 1931 will be awarded early in January to Dr. Arthur D. Little, president of the chemical research and testing laboratories bearing the same name. This honor is conferred yearly upon a distinguished chemist on the basis of a life-time achievement in chemistry by the joint action of five chemical societies. The first medal was given to Sir William Perkin in 1906, and last year it was awarded to the late Dr. Herbert H. Dow. The five societies whose representatives select the recipient are: Society of Chemical Industry, American Chemical Society, American Electrical Society, American Institute of Chemical Engineers and the Société de Chimie Industrielle.

Dr. Little is past president of the American Chemical Society, of the American Institute of Chemical Engineers and of the Society of Chemical Industry. In the latter capacity he presided at the annual general meeting of the society at Manchester, England, in 1929, at which time the University of Manchester conferred on him the honorary degree of doctor of science and the Manchester College of Technology made him an honorary associate.

Dr. Little has worked extensively in the field of the application of chemistry to industry. He is the inventor of processes for the manufacture of chrome-tanned leather, chlorate of potash, cellulose acetate, smoke filters, newsprint from Southern woods, as well as others dealing with the recovery of naval stores from lumbering wastes and vapor-phase cracking of petroleum. He has made a specialty of the chemistry of cellulose fibers and paper-making, and of gas and petroleum.

In 1922, as chairman of a committee of the American Institute of Chemical Engineers, he made a thorough study of courses of chemical engineering in American institutions of learning and prepared an extensive report covering his findings. He has at all times maintained that the training of chemical engineers should be broad and cultural, as well as technical, if they are to be fully equipped to reach their highest development personally as well as professionally.

Dr. Little has been untiring in his endeavors to place chemistry on a par with the other recognized professions and he has unceasingly urged upon the practitioners of chemistry the importance of upholding the profession and of maintaining for themselves a proper place in the community. He is able to translate the findings of chemistry to the layman in a clear and readable style, and he has used this gift unstintingly in the service of chemistry. From the beginning of his career he has been one of the country's effective exponents of research, and he has helped in no small measure to bring about the better appreciation of the creative power of research which has come in recent years.



THE JUNGFRAU AS SEEN FROM INTERLAKEN

A SCENE ON TOP OF THE JUNGFRAU

SHOWING THE OLD METEOROLOGICAL PAVILION AND THE MASSIVE SNOW BANKS.

THE HIGH ALTITUDE INSTITUTE ON THE JUNGFRAU

A NUMBER of organizations have co-operated in bringing to fruition plans for the Jungfrau High Altitude Institute (Institut für Höhenforschung auf dem Jungfraujoch) in Switzerland. The building of the Jungfrau railroad made possible access by tunnel at all seasons of the year to a mountainous area at a height of 3,500 meters. A site on the Jungfrau in the vicinity of the railway terminus was chosen for the scientific institute where investigators in the field of astronomy, botany, geology, meteorology, glaciology, hydrology, physics, physiology and biology will have facilities for pursuing individual investigations.

In addition to support from the Cantonal Government, the Jungfrau Railway and other local sources, aid toward this project was obtained from such varied international sources as the Kaiser Wilhelm Society, the Royal Society of London, the Academy of Sciences of Vienna, the University of Paris and the International Education Board.

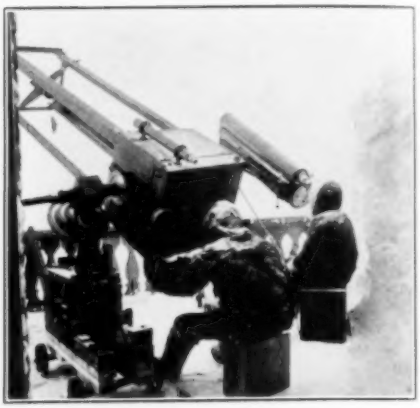
THE JUNGFRAU HIGH ALTITUDE INSTITUTE

UNDER CONSTRUCTION IN SWITZERLAND.

THE OLD METEOROLOGICAL PAVILION

The latter organization authorized a contribution for this purpose in 1928. It is understood that the University of Geneva expects to erect a small astronomical observatory in the vicinity.

The development of the institute has been under the control of a committee of the Swiss Society of Natural Sciences and of the Kaiser Wilhelm Society, under the presidency of Professor W. R. Hess. A board of directors is elected from the membership of the Swiss Society of Natural Sciences and forms the administering body. The use of the station is available to scientists of all nationalities. It will provide facilities for work in the natural sciences with problems whose solution depends upon experimental observations made at a high altitude. It is realized that research in the natural sciences requires easy and sure connections with the rest of the world. In the new building modest but adequate facilities for many-sided activities will be available throughout the year.



ASTRONOMERS AT WORK

ASTRONOMICAL OBSERVATIONS ARE REGULARLY MADE ON THE MOUNTAIN TOP.

The Berghaus Jungfraujoch on the summit has the distinction of being at a higher altitude than any hotel in Europe. Easily accessible by electric mountain railway, the Jungfrau has become throughout the year a playground for lovers of snow sports. The establishment of the new institute will provide an added interest to tourists.

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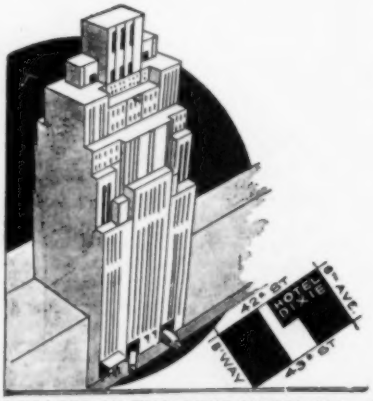

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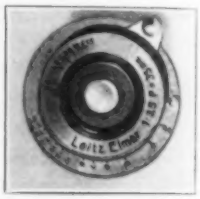

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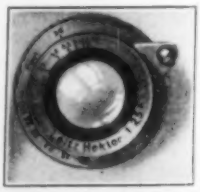

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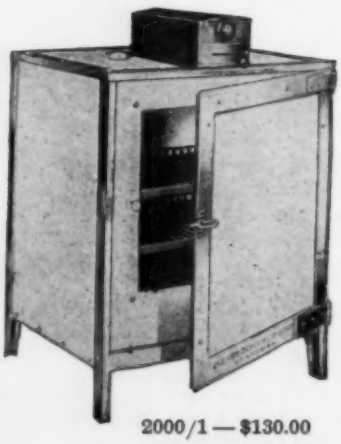

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